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A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

VOL. X.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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1805.

PREFACE.

THE Authors of Original Papers in the present Volume, are Sir H. C. Englefield, Bart. M. P. F. R. S.; Mr. H. Steinhauer; Mr. John Gough; Ra. Thicknesse, Esq.; Mr. Wm. Wilson; C. Wilkinson, Esq.; J. Whitley Boswell, Esq.; Mr. W. Brandé; Aletes; Mr. John Dalton; Capt. J. Mortlock; W. F. S.; Mr. Charles Sylvester; Mr. Ezekiel Walker; T. B. C.; Jos. Huddart, Esq. F. R. S.; E. O.; W. B.; Mr. N. Mendelshon; C. L.; Mr. Accum; Right Hon. Sir Joseph Banks, Bart. P. R. S. &c. &c.; Mr. R. Matthews; Mr. J. Biddle; An Old Correspondent.—Of Foreign Works, Thenard; Hesinger; Bergelius; Morozzo; Bucholz; Dartigues; Schoerbing; Boullay; Lagrange; Lussac; Erman.—And of English Memoirs, abridged or extracted; Smithson Tennant, Esq. F. R. S.; W. H. Wollaston, M. D. F. R. S.; C. Hatchett, Esq. F. R. S.; Gregory Watt, Esq.; Right Hon. C. Greville, V. P. R. S.; George Pearson, M. D. F. R. S.; J. Hinckley, Esq.; F. S. A.; T. A. Knight, Esq.

Of the Engravings the Subjects are, 1. An ancient Egyptian Engraving in Symbols, supposed to be astronomical. 2. Figures to illustrate the Galvanic Charge, by C. Wilkinson, Esq. 3. Outlines of Pendulous Measures of Time, by J. W. Boswell, Esq. 4. Figure to illustrate Mr. Gough's Theory of the Augmentation of Sound, by communicated Vibrations. 5. Instrument for surveying, by Capt. J. Mortlock. 6. Instrument for taking Designs from Nature. 7. Plan and Sections of an improved Air-Pump, by Mr. Mendelsshon. 8. Figures to illustrate Mr. Gough's Theory of

the

the Speaking Trumpet. 9. Improvement in Woulfe's Apparatus. 10. Method of drawing a Line in a Circle equal to the Side of its Square. 11. Two quarto Plates, exhibiting highly magnified Representations of the Parasitic Plant which causes the Blight in Corn, from Drawings by M. Bauer, Botanical Painter to the King. 12. Mathematical Diagrams of the Properties of Triangles, by Mr. Gough. 13. Application of Spectacle Glasses, by Mr. E. Walker.

Soho Square, April, 1805.

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A

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NATURAL PHILOSOPHY, CHEMISTRY,

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JANUARY, 1805.

ARTICLE I.

Concerning the original Inventors of certain philoſophical Diſcoveries: the Reflection of Cold; Compreſſion of Water in a metallic Veſſel; the Teleſcope; and a perſpective Inſtrument formerly deſcribed. In a Letter from Sir H. C. ENGLEFIELD, *Bart. M. P. F. R. S. &c. &c.*

To Mr. NICHOLSON.

SIR,

ALTHOUGH abſtractedly ſpeaking, it is of no importance to ſcience who firſt made the moſt valuable experiments, or to what individuals the world is indebted for the moſt uſeful diſcoveries; yet as the ſenſe of mankind has univerſally in this, as in ſeveral other things, been in direct oppoſition to frigid reaſoning, and every one has been deſirous to vindicate the fame of eminent men, by aſcertaining to them the honour due to their labour or genius, I truſt that the following obſervations will not be deemed unworthy a place in your valuable Journal.

Univerſal ſentiment in favour of diſcoverers.

1. The experiment of the reflexion of cold has been, as far as I know, both in converſation and in printed books, uniformly aſcribed to Mr. Pictet of Geneva. Now in the collec-

Reflection of cold aſcribed to Pictet.

It was before made by the Acad. del Cimento.

tion of experiments publiſhed by the Academy Del Cimento, of Florence, in the year 1666, the identical experiment is deſcribed in the following words:

" *Ninth Experiment.*

Narrative.

" We were deſirous to try whether a concave ſpeculum, expoſed to a maſs of ice weighing 500 pounds, would reflect any ſenſible degree of cold on a very delicate thermometer of 400 degrees, placed in it focus. The reſult was, that the thermometer inſtantly ſunk: but a doubt remained whether the thermometer was acted on more by the direct cold of the ice, or that reflected by the ſpeculum. This doubt was removed by covering the ſpeculum: and certain it is (whatſoever might be the cauſe) that the ſpirit inſtantly began to riſe again. Yet ſtill we will not preſume poſitively to affirm, that this riſe might not have been owing to ſome other cauſe than the taking off the reflection from the ſpeculum, all the precautions not having been taken which might be conſidered neceſſary to ſecure abſolute aſſent to the experiment."—*Saggi di Naturali Eſperienze, page* 176.

I do not mean to aſſert that Mr. Pictet had ſeen this moſt excellent book, or that he borrowed from it without acknowledging his obligation; but the honour of having firſt made the experiment is certainly due to the Florentine philoſophers.

Excellency of their publication.

It may not be foreign from the ſubject here to obſerve, that the " Saggi," for perſpicuity, brevity, and that diffident caution ſo eſſential to the inveſtigation of truth, is a model of philoſophical writing, not perhaps excelled by any book written ſince that time, and more admirable when we conſider the diffuſe and obſcure ſtile ſo much in faſhion in the works of the learned at that period.

The Acad. del Cimento ſaid, in moſt books, to have compreſſed water in a globe of gold.

2. Scarcely any treatiſe on natural philoſophy has failed to quote an experiment made by the academicians Del Cimento, on the incompreſſibility of water incloſed in a globe of gold, and ſubmitted to the preſſure of a ſcrew preſs. Mr. Canton, in his experiments on the ſame ſubject, publiſhed in the 52d vol. of the Phil. Tranſ. ſo ſpeaks of it. You will probably be ſurpriſed when I aſſert, that the academicians did not try the experiment with a gold globe, but one of ſilver; and that they give a reaſon for uſing ſilver in preference to gold, namely, that the ductility of gold was ſo great that it would extend

The globe was ſilver, and compreſſed by hammering.

extend its dimenſions by preſſure, and ſo defeat the intention of the experiment. Neither was the globe compreſſed by a machine, but merely by hammering.

Lord Bacon firſt made this experiment.

But the honour of this experiment is not due to the Florentine philoſophers, but to our own illuſtrious countryman Lord Bacon, who, in the *Novum Organum*, Book II. Sect. 45, gives the very ſame experiment on water encloſed in a large and ſtrong globe of lead, which being firſt compreſſed by the hammer and then by a ſcrew-preſs, the water exuded through the pores of the metal, and ſtood in a dew on its ſurface.

The inaccuracy with which this experiment has been related, though in this caſe of no great importance, may however be an uſeful leſſon, and induce thoſe who wiſh to attain to truth, to truſt as little as poſſible to information at ſecond-hand, but to recur to the original authority whenever it is practicable to do ſo.

The teleſcope was uſed by Kepler in aſtronomy, two years before Galileo.

Before I conclude, permit me to mention a curious circumſtance relative to the æra of the invention of the teleſcope. Almoſt all books place it in the year 1609, and the firſt diſcoveries of Galileo were made in 1610; yet Kepler, in his account of the comet which appeared in September, 1607, (*See Kepler de Cometis, page* 25) expreſsly ſtates, that though, to the naked eye, it was only equal to certain fixed ſtars near it, yet, when viewed with teleſcopes (*perſpicillis*), it appeared larger than thoſe ſtars.

Remarks on the progreſs of diſcovery, &c.

Although the intercourſe between the different nations of Europe was at that time much leſs than it now is, it is very difficult to ſuppoſe that Galileo ſhould not, in upwards of two years, have heard, not only of the invention, but its application to celeſtial obſervations by a man ſo eminent in ſcience as Kepler: and it is ſcarcely leſs extraordinary that Kepler himſelf ſhould not, in that ſpace of time, have foreſtalled Galileo in ſome of thoſe diſcoveries which rendered his name ſo illuſtrious; for however imperfect Kepler's teleſcope might be, it is hardly poſſible that it could have failed to ſhew him the ſatellites of Jupiter, which are viſible to the commoneſt opera glaſs.

I am, Sir,

Your obedient ſervant,

H. C. ENGLEFIELD.

Tilney Street, Nov. 30, 1804.

Ramſden was the inventor of a teleſcopic perſpective inſtrument referred to.

P. S. I beg leave to add, that the machine for drawing in perſpective, deſcribed in your Journal for October, page 122, was (I am almoſt abſolutely certain) the invention of the late moſt eminent Mr. Ramſden, and was made by him full 25 years ſince, for the Right Honourable Charles Greville.

II.

Account of an ancient Egyptian Sculpture with Hieroglyphics, ſuppoſed to relate to Aſtronomy. In a Letter from Mr. H. STEINHAUER. *With an Engraving.*

To Mr. NICHOLSON.

SIR, *Fulneck, Oct. 25, 1804.*

Porphyry ſculptured in the form of a ſcarabæus, and bearing hieroglyphics.

PERHAPS the liberty that I take in ſubmitting the annexed draught of an Egyptian antiquity, may be an intruſion upon the plan of your valuable Journal. The Egyptian hieroglyphic inſcriptions are involved in ſuch impenetrable darkneſs, that every trifle contributed to their elucidation cannot but be acceptable, indeed equally acceptable to the antiquarian, the hiſtorian, and the aſtronomer; ſince the greater part have probably a reference to that ſcience. Stones ſculptured in the ſhape of a ſcarabæus are, I believe, (for I make no pretenſions to the title of an antiquarian) well known; there are a few in the Britiſh Muſeum; but I never yet ſaw either a facſimile of the inſcription, or any attempt at an explanation of the ſame. The drawings incloſed are taken from a ſtone brought by the Swediſh conſul at Aleppo to Stockholm, and there preſented by him to a gentleman, who has favoured me with the loan and permiſſion to make drawings or impreſſions of it at pleaſure. The upper part, of the ſize of the ſketch, obviouſly repreſents a ſcarabæus; probably the S. Sacer, Lin. & Fabr. though the indentures on the head are but ſlightly marked, and a ſlight damage between the thorax and elytra render it impoſſible to diſcover whether a ſcutellum be indicated or not. It was perhaps the only beetle they repreſented in ſculpture, and the accuracy of a modern nomenclator is conſequently unneceſſary to determine it. If I recollect right, this

this beetle was made uſe of as a ſymbol of the annual rotation of the ſun. The under ſide is flat, or ſlightly convex, perhaps from wear, and bears eight lines of hieroglyphics, probably referring to the change of the ſeaſons. The drawing, made with the greateſt accuracy from an impreſſion in ſealing-wax, is conſequently inverted.* Should you, ſir, or any of your friends, wiſh for one to prove the exactneſs of the draught, it is very much at your ſervice. The ſtone itſelf is apparently a ſpecies of porphyry, the *ſerpentino verde antico*, of a green maſs, with very dark green field-ſpar, and pale green indiſtinct ſtripes. The plainneſs of the inſcription, and the ſame ſymbol occurring ſeveral times upon it, led me to ſuſpect, that it would be no very difficult taſk, for ſuch as are verſant in antiquities of the kind, to diſcover ſomewhat of its purport; and being conſcious of its value, I was unwilling to let it lie by me, without offering it to the attention of ſuch as are more deeply intereſted in it. Your wiſh to promote the arts and ſciences, which ſo obviouſly pervades your eſteemed Repoſitory, makes me hope that you will excuſe mine, in caſe the ſubject ſhould not be admiſſible into your Publication.

Porphyry ſculptured in the form of a ſcarabæus, and bearing hieroglyphics.

I remain, Sir,

Your obedient humble ſervant,

H. STEINHAUER.

P. S. Is the carbonate of barytes a frequent Britiſh mineral, except at Chorly? If not, it may perhaps be intereſting information, that it is found in very large quantities near Murton in Cumberland, and ſome other places of the vicinity. The native oxide of zinc, ſcarcely inferior to the flowers of zinc of the ſhops, I do not recollect to have ſeen mentioned. It is found in one part of the Weſt Riding of Yorkſhire, in conſiderable quantities.

Native carbonate of barytes, and oxide of zinc.

* The engraver having followed the drawing upon the copper, our print is not inverted.—N.

Extract

III.

Extract of a Memoir on the fuming Liquor of CADET. *By Cit.* THENARD.*

History of the discovery of Cadet's fuming liquor, &c.

CADET discovered this liquor near half a century ago, in making investigations on arsenic. The name of its author was given to it then, which it has retained ever since, because its intimate nature and constituent principles were unknown. The thick smoke which this singular product spreads through the air, its specific gravity, greater than that of water, its oily state, its great volatility, its powerful odour, its spontaneous inflammation in the air, observed by Cadet and the chemists of Dijon; in fine, all its properties, each more extraordinary than the other, induced Cit. Thenard to subject it to analysis.

Method of obtaining it. Distillation of acetite of potash and arsenious acid.

He began by procuring several ounces of this liquor, by distilling, according to Cadet's process, equal parts of acetite of potash and arsenious acid, the produce of which he received in glass globes, cooled with a mixture of ice and marine salt. A liquor, very little coloured, and smelling strongly of garlic, soon passed into the receivers; at the same time much gas was disengaged, which spread the same odour, and the receivers were filled with vapours, so heavy that they seemed to run like oil. When the operation was terminated, he unluted the apparatus, and broke the retort. The bottom of it was covered with a white, acrid, and alkaline substance of potash, arising from the acetite employed; and the neck was lined with crystals of arsenic, owing to the reduction of the arsenious acid. The gases, the quantity of which was very great, contained arseniated hidrogen, in addition to the carbonated hidrogen and carbonic acid, given by all vegetable matters which are decomposed by fire. The liquid product was composed of two very distinct strata, holding metallic arsenic in solution, which was not long before it was deposited in flakes; the upper one was of a browniſh-yellow and aqueous, the lower one was less coloured, and of an oily appearance. He separated them, by pouring them into a tube drawn to a point by the lamp, which allowed him to receive them

Residue.

The liquid product consists of two distinct fluids,

* From Bulletin des Sciences, Tom. III. p. 202.

in

in different veſſels. The heavieſt, as being the moſt uſeful to be known, was firſt examined. At the commencement he was affected by the thick vapours which it diſperſes through the air, and by its extremely penetrating and horribly fetid odour. Its action on the animal economy is ſo powerful, that he found it impoſſible to devote more than one hour in a day to his enquiries, and he was more than once tempted to abandon them. He was in the ſame ſtate as if he had taken a ſtrong medicine, and experienced ſtupefying effects; againſt which he employed ſulphurated hidrogen diſſolved in water, with ſucceſs.

Examination of the heavieſt.

It powerfully affected the animal economy.

As he had but a ſmall quantity of the liquor at his diſpoſal, and it was of importance that his attempts ſhould not be fruitleſs, he regulated the order of his enquiries in this manner: He firſt determined the cauſe of the odour which it ſpreads through the air; then he ſought for that of the thick vapours which it produces, and afterwards that of its ſpontaneous inflammation; and he made uſe of the determination of theſe three points to diſcover the fourth, and moſt important, the conſtituent principles of the ſubſtance.

Order of the inveſtigation.

The odour could only proceed from the ſubſtance itſelf, or elſe from an elaſtic fluid which it might hold in ſolution, and which the author ſuppoſed to be arſeniated hidrogen. He therefore diſtilled, with great care, a certain quantity of the liquor in a ſmall glaſs retort, to which were adapted a receiver and a tube for collecting the gaſes. He obtained nothing but the air of the veſſels; the liquor was entirely volatilized, and paſſed into the receiver without having undergone any alteration, except that its colour was not quite ſo deep. Hence the odour of the arſenical liquor is owing to its property of being volatilized, and, probably, diſſolved in the air.

Cauſe of the odour.

The liquor was unchanged by diſtillation.

The cauſe of its vapours could only be owing to an abſorption of oxigen, or to an abſorption of the water diſſolved in the air, or to both theſe effects at the ſame time. The air of a flaſk, into which Cit. Thenard had poured a few drops, immediately loſt its tranſparency, and, in a ſhort time, became incapable of ſupporting the combuſtion of a taper. A veſſel of the ſame ſize, filled with carbonic acid, offered the ſame phenomena, but in a leſs diſtinct manner. To guard againſt the contact of the air, he had been careful to ſuſpend a very thin tube, containing the liquor, to the cork of the flaſk, ſo

Its vapour is occaſioned by an abſorption of oxigen and water.

ſo that he could eaſily break it againſt the ſides of the veſſel. The vapours were not perceptible when he uſed carbonic acid perfectly dried; whence he concluded, that the vapours of the arſenical liquor are owing to an abſorption of the oxigen and the water contained in the air jointly, but that the firſt of theſe cauſes ſeems to be more powerful than the ſecond.

Its property of inflaming ſpontaneouſly is owing to metallic arſenic.

From this it ſhould ſeem that the arſenical liquor is poſſeſſed of the property of inflaming alone. It does not however take fire at the approach of a body in combuſtion, when it is very pure; and it is obſerved, that, in all the ſpontaneous inflammations which it experiences, the combuſtion always begins at the black ſpecks which diſturb its tranſparency, and are only metallic arſenic very much divided.

It muſt therefore contain arſenic,

It now remained to determine the nature of the arſenical liquor. Its odour, which reſembles arſeniated hidrogen gas, indicated that it muſt contain arſenic, and that this metal muſt have great influence on the phenomena which it offers: Its combuſtibility, its conſiſtence, and its appearance, indicated an oily matter; and, although it did not change the tincture of turnſole, and no re-agent demonſtrated immediately the exiſtence of acetous acid, this body muſt nevertheleſs be expected in it. To ſucceed in inſulating theſe different ſubſtances, the author tried the alkalies; but experience ſoon convinced him that he muſt have recourſe to other means. He employed the oxigenated muriatic acid with much greater ſucceſs. Some drops of the liquor, poured into this gas, were inſtantly inflamed, and their decompoſition was complete. They were then precipitated in white flocks by lime-water, and in yellow ones by ſulphurated hidrogen; while, on being ſaturated with potaſh and evaporated, they formed a foliated ſalt, ſtrongly attracting the humidity of the air, acrid, ſharp, decompoſable by ſulphuric acid, and diſengaging a ſtrong odour of vinegar. The quantity of arſenic and of acetous acid obtained, being far from correſponding with the quantity of liquor which had been uſed, there muſt therefore have been another body in it which it was requiſite to inſulate, and this was accompliſhed by treating a new portion of the liquor with a quantity of water ſufficient to diſſolve it; then on decompoſing it by ſulphurated hidrogen, it yielded a precipitate, ſlightly yellow, very much divided, formed principally of arſenic and ſulphur, which was a conſiderable time in ſepa-

an oily matter,

and acetous acid; but this was not indicated by alkalies.

Oxigenated muriatic acid decompoſed it.

Formed a foliated ſalt with potaſh.

Its ſolution in water, decompoſed by ſulphurated hidrogen, yielded ſulphur, arſenic, and oil, containing acetous acid.

rating

rating from an oil, which was afterwards ſeen ſwimming on the ſurface of the liquor. This contained a great deal of acetous acid. Its decompoſition may be further facilitated by expoſing it to the air; then it emits thick vapours, it is cryſtallized, and becomes ſlightly humid. It is rendered turbid by lime-water, and yields a yellow precipitate with ſulphurated hidrogen.

From theſe different experiments it follows, that this liquor is compoſed of oil, acetous acid, and arſenic, nearly in a metallic ſtate; and that it muſt be conſidered as a ſpecies of ſoap, with a baſe of acid and arſenic, or as a ſort of oleo-arſenical acetite. Compoſition of the heavieſt liquor.

This analyſis was very uſeful in that of the upper liquor. In fact, notwithſtanding the difference which ſeems to exiſt between them, ſince the latter reſembles water, can combine with it in all proportions, forms only a ſlight cloud in the atmoſphere, has much leſs odour, and does not inflame in any caſe, it is eaſy to prove that it only differs from the firſt by its greater proportion of acetous acid, and by the water which it contains; for it reddens the tincture of turnſole ſtrongly, effervesces with the carbonates, gives riſe to acetites, and is ſlightly precipitated of a yellow colour by ſulphurated hidrogen, which ſeparates a little oil from it. A very ſmall quantity of oxigenated muriatic acid ſpeedily deſtroys the odour, and it is then precipitated white by lime-water, and of a deep yellow by the hidro-ſulphurets. Expoſure to the air produces in it, by time, the ſame changes as take place immediately with oxigenated muriatic acid. Finally, a liquor, exactly ſimilar, is formed by diſſolving a few drops of the lower liquor in very weak vinegar, and thus ſyntheſis confirms the reſults of analyſis. The upper fluid differs from the lower, only by having a larger proportion of acetous acid and water.

We can now eſtabliſh a theory, clear of all hypotheſis, on the phenomena offered by the diſtillation of acetite of potaſh and arſenious acid; we ſee that one part of the arſenious acid is entirely reduced; that another is only brought near the metallic ſtate; that the acetite of potaſh is totally decompoſed; that almoſt all the acetous acid is alſo decompoſed; and, that from theſe different decompoſitions reſult water, carbonated hidrogen, arſeniated hidrogen, carbonic acid, a peculiar oil, oxide of arſenic, arſenic, and potaſh; that the potaſh forms the white reſidue found in the veſſels in which the diſtillation Theory of the proceſs. Component parts of the fuming liquor.

was

was performed; that the arſenic is ſublimed, and adheres to the neck of the retort; that the three different ſpecies of gas are mixed, and may be collected in flaſks; finally, that the water, the oil, the acetous acid, and the oxide of arſenic, are condenſed in the receiver; that theſe three laſt bodies, by combining in certain proportions, form a very volatile compound, heavier than water, and ſparingly ſoluble in it; and that it is for this reaſon that it ſeparates into two very diſtinct ſtrata; the lower of which muſt be conſidered as an oleo-arſenical acetite, and the other as a portion of the firſt diſſolved in water, which ſolution is promoted by an exceſs of acetous acid.

IV.

Account of Cerium, a new Metal found in a mineral Subſtance from Baſtnas, in Sweden. *By* W. D'Hesinger *and* J. B. Bergelius.

(Concluded from Page 300, Vol. IX.)

With Succinic Acid.

Succinate of cerium.

(P.) At firſt, ſuccinic acid renders the ſaturated ſolutions of cerium turbid, but afterwards they become clear. Thus, a few drops of ſuccinate of ammonia, poured into a ſaturated nitric or muriatic ſolution of this metal, occaſion a precipitate, which ſoon diſappears. On pouring in a greater quantity of this re-agent, the ſuccinate of cerium is immediately depoſited. This combination is white; it is not wholly inſoluble in water, ſince the ſolution from which it was ſeparated ſtill retains a little, as is manifeſted by evaporation, or by alkalies. The ſalt obtained by digeſting free ſuccinic acid with the oxide, comports itſelf in the ſame manner. The acid diſſolves it readily. Expoſed to the fire, it burns with a blue flame.

Since the acetate of cerium is not precipitated by the ſuccinate of ammonia, this is a certain means of obtaining the cerium freed from iron.

With Gallic Acid.

Gallate of cerium.

(Q.) If cryſtallized gallic acid is put into a ſaturated ſolution of muriate of cerium, a ſmall quantity of a white precipitate

is

is formed. The alkalies augment it, and give it a clear chocolate colour. If a larger quantity of alkali is gradually added, the quantity of the precipitate, and the intenſity of its colour are increaſed; in proportion as this addition is made, the precipitate becomes of a reddiſh brown, and at length, by expoſure to the light, gives a turbid, deep green ſolution.

With Pruſſic Acid.

(R.) The ſaturated ſolutions of cerium are precipitated white by the pruſſiates. The voluminous precipitate has much reſemblance to ſilver precipitated by muriate of ſoda. An exceſs of acid readily diſſolves this precipitate. Pruſſiate of cerium.

Oxide of Cerium with Sulphurated Hidrogen.

(S.) At the commencement, the ſaturated ſolutions of cerium are precipitated of a browniſh colour by hidro-ſulphuret of ammonia; but if more of this re-agent is added, the precipitate is of a deep green. The muriate of cerium alone takes a deep green colour, but if an alkali is added to it, an hidro-ſulphurated combination of muriate of cerium, at a *minimum* of oxidation, is formed, which is rapidly precipitated of a bright green colour. Hydro-ſulphuret of cerium.

The hidro-ſulphurated cerium, well dried, has a deep green colour, almoſt black. It is eaſily deſtroyed by heat; put into a crucible, moderately hot, it burns with a yellowiſh, phoſphoric flame, only viſible in the dark.

Sulphurated Oxide of Cerium.

(T.) The ſulphuret of ammonia gives a fading brown precipitate with the ſolutions of cerium. This ſulphuret, added in exceſs, gives a precipitate of a graſs green, which becomes bright green by deſiccation: it burns with a blue flame and the pure oxide remains. Sulphurated oxide of cerium.

The muriate of cerium at a *minimum* is precipitated white by ſulphuret of ammonia.

Oxide of Cerium with Phoſphorus.

(U.) A piece of pure phoſphorus was put into a ſolution of muriate of cerium contained in a cloſed veſſel, and kept for ſeveral days on a ſtove. The bottom and ſides of the veſſel were covered with a white precipitate, and the piece of phoſ- Phoſphate of cerium.

phorus

phorus was covered with a hard brown cruſt, from which the phoſphorus was ſeparated by heating it in warm water. This cruſt was tenacious, and ſhining in the dark. Heated, it took fire like phoſphorus, and left a reſidue of a ſmall quantity of oxide, which, on being melted with borax, exhibited the characters deſcribed above, except that the globule retained its clear green colour, after being cooled. It appears probable that theſe phenomena aroſe from the phoſphorus containing cerium. But the phoſphorus, kept for a month in the ſame ſolution, ſlowly depoſited a white powder; whence it is uncertain whether the formation of the cruſt, mentioned above, was owing to the cerium. The acid was always in exceſs in the ſolution, and the white powder had all the appearance of phoſphate of cerium.

With the Alkalies.

Action of the alkalies.

(V.) The pure alkalies do not diſſolve cerium, even by fuſion. By this means it is eaſily deprived of manganeſe. Pure ammonia digeſted with the oxide, does not diſſolve it, but renders it yellowiſh.

The carbonated alkalies diſſolve the oxide of cerium in ſmall quantity. The ſolution is yellow, and is precipitated by the acids. They alſo diſſolve it by fuſion in covered veſſels. In the open fire, it oxides too much to enter into ſolution.

Iron or zinc do not precipitate the ſolutions of cerium.

The ſolutions of cerium are not precipitated by iron or zinc.

Action of galvaniſm.

The electric pile of Volta only decompoſes the ſalts, and the yellow oxide adheres to the conductor.

V.

New Experiments on Abſorption by Charcoal, made by Means of a new Machine. *By* C. L. Morozzo.

(Concluded from P. 264 Vol. IX.)

On Oxigen Gas.

Oxigen gas.

25th. I NEXT proceeded to the examination of the abſorption by charcoal in oxigen gas.

I therefore extracted ſome oxigen gas from red precipitate to be employed in my different experiments. Of this gas, trie

tried with charcoal in my machine, eight inches and ſix lines were abſorbed: this great abſorption ſurprized me; for in my firſt experiments, in 1783, made in cloſed tubes, I had had but a very ſmall one: I therefore found no difficulty in attributing it to my matraſs having been melted towards the cloſe of the operation, which made me ſuſpect that my gas had been ſpoiled by the nitrous acid; and I repeated the experiment with great care. Oxigen gas.

26th. I then took oxigen gas which had been obtained from red precipitate with great attention, and had been prepared in the laboratory of Doctor Bonvoiſin; what was my aſtoniſhment at obſerving that, in thirty-ſix hours, the abſorption amounted to twelve inches and eight lines, and that it continued for ſeveral days: in forty-eight hours it was fourteen inches ſix lines; in four days, fifteen inches two lines; in five days, ſixteen inches four lines; and, finally, in three days more, the gas was entirely abſorbed.

27th. This experiment was too intereſting to omit repeating it with the greateſt poſſible preciſion.

I therefore took ſome of the ſame oxigen gas, and the abſorption was twelve inches three lines in the firſt four hours; in forty-eight hours, it was thirteen inches ſix lines; in three days, fourteen inches five lines; at the end of the fourth day, it was fifteen inches three lines? of the fifth, ſixteen inches; of the ſixth, ſixteen inches ſeven lines; of the ſeventh, ſeventeen inches two lines; finally, on the eighth day the gas was entirely abſorbed.

28th. I repeated the ſame experiment on oxigen gas obtained from water expoſed to the ſun, in which I had put three ounces of charcoal-powder *; in this gas, which was very pure, the abſorptions took place in the ſame manner.

29th. Theſe facts will ſurpriſe thoſe who, having made their experiments on oxigen gas in tubes cloſed hermetically, and by paſſing the charcoal through the mercury, obtained only very ſmall abſorptions, which never exceeded three inches

* In the twelfth volume of the Italian Society, I have inſerted a memoir on the property which charcoal mixed with water has of developing one-third more of oxigen gas, of a ſuperior quality to that obtained from pure water: In this memoir I prove, that a part of this oxigen gas is produced by the charcoal, which communicates the *principle of fire* to the air contained in the water.

Oxigen gas.

three lines in tubes of twelve inches in height. I am certainly not miſtaken; and M M. Rouppe and Van-Norder, who have repeated my experiments in their apparatuſes, have obtained the ſame reſults, and have found, as I did, that, next to hidrogen, oxigen gas is that which is leaſt abſorbed by charcoal.*

30th. I muſt not paſs over a ſingular experiment, the explanation of which will alſo be very difficult.

I examined the abſorption which a piece of charcoal, that had remained ſeven hours in the bright light of the ſun, had effected in the ſame oxigen gas; it was only between ſeven and eight lines, while a ſimilar piece, which had been only five hours in the ſolar light, produced an abſorption of ten inches three lines in carbonic acid gas.

It muſt be obſerved, that a piece of charcoal expoſed to the light of the ſun and placed in oxigen gas, produced only an abſorption of ſeven or eight lines, juſt like the abſorption of a piece which had been expoſed to the ſun's light, and placed in hidrogen gas, which gave only an abſorption of ſix lines: and, in this, theſe reſults are perfectly conformable to my experiments, made in the year 1783.

31ſt. This difference of the reſults is therefore only owing to the method either of paſſing the charcoal under the mercury, or of leaving it in the machine. It appears that, by paſſing it through the mercury, the charcoal loſes much of its attractive power, which it retains in the machine.

Let us endeavour to find whether theſe differences can be accounted for.

32d. It might be ſuppoſed that the red-hot charcoal inflames the oxigen gas; but ſince I do not open the key until ſome time after the charcoal has been inſerted, it does not appear to me to be likely that the gas can be inflamed: beſides, if this were the caſe, the abſorption would be made at once, and it would not require eight days to be complete.

33d. It might be ſuppoſed that, in this caſe, the charcoal ſupplied hidrogen gas, which, mixing with the oxigen gas and producing water, would cauſe the abſorption: but during the whole experiment there was not the ſmalleſt drop of water perceived in the tube.

* Ann. de Chimie.

34th. There might be reaſon to believe that the charcoal, by inflaming the oxigen gas, might convert it into carbonic acid gas, which is the moſt abſorbed of all the gaſes; but to ſatisfy myſelf of this, I produced an abſorption of oxigen gas by a piece of charcoal, as in the former experiments: in twenty-three hours it amounted to twelve inches, then with a ſyringe of cryſtal I paſſed ſome lime-water through the mercury into the remaining gas, and it did not become turbid, which proves that carbonic acid was not formed; and, having turned up the tube, the reſidual gas extinguiſhed a taper: I believe it to have been azote.

35th. I was of opinion that incandeſcent charcoal would abſorb more than another piece which had been ſuffered to cool till it no longer appeared red. Experiment ſhews a greater abſorption in the firſt for twenty-four hours; but, leaving the apparatus undiſturbed, in two days the abſorption became equal. I operated upon atmoſpheric air.

36th. I muſt acknowledge my inability to give an explanation of theſe ſingular experiments, as I have already declared above: were I to hazard any conjectures, they would probably be overturned by new theories, which do not ſpare even the labours of the greateſt chemiſts in France.

37th. I ſhall therefore continue my operations as I propoſed, and new experiments will, at leaſt, ſupply the want of ſyſtems.

On the Difference in the Charcoals made uſe of.

38th. Having aſcertained the abſorptions effected by the charcoal of beech wood in atmoſpheric air and in the different gaſes, I proceeded to examine the variations which the charcoals of different woods would produce in them. Difference in the charcoals made uſe of.

39th. I therefore took two pieces of charcoal, the one of beech, the other of the branches of ſallow deprived of bark: I introduced them both in an incandeſcent ſtate, and examined the difference of their abſorption in the ſame atmoſpheric air. Charcoal of ſallow;

That of beech gave an abſorption of ſeven inches and eight lines in ſix hours.

That of ſallow gave only an abſorption of four inches and three lines, and there was no further abſorption after ſix hours.

Theſe

Theſe experiments correſpond perfectly with thoſe which I have publiſhed in my third memoir. I ſhall diſpenſe with the relation of ſeveral other experiments which I have made on different woods, ſuch as hazle, vine-twigs, &c. all of which gave leſs abſorptions.

of box; I afterwards tried the charcoal of box wood, although it is a very compact wood; but ſince Kirwan found that this wood contains much more ſaline matter than any other wood, it was deſirable to ſubmit it to experiment: its abſorption, however, was conſiderable, and equal to that produced by beech-wood. The greater or leſs quantity of ſaline matter in the wood of which the charcoal is made, therefore, does not contribute to the variations in the abſorption.

of cork. 40th. I afterwards examined the charcoal of cork, which intereſted me greatly, becauſe Cit. Odier, from the experiments of Dr. Beddoes, has propoſed it as a medicine: I was deſirous of examining whether it contained more of the principle of fire than the others. I therefore charred cork; but it is ſo light, that, after the charring, a piece of equal bulk with that of the beech which I had uſed, weighed only three grains: to bring the circumſtances to an equality, it was therefore neceſſary to make the experiment with a piece of beech charcoal which weighed three grains*: it was in atmoſpheric air it was tried. The following are the reſults:

Three grains of beech charcoal produced an abſorption of one inch and nine lines.

The charcoal from cork abſorbed only nine lines.

The charcoal of cork contains more of the principle of fire than any other charcoal. 41ſt. According to my principles†, the charcoal of cork muſt contain the matter of fire more than any other charcoal; thus when it is neceſſary to give it as a medicine, either inwardly or outwardly, it ſhould be preferred to all the others; and I am of opinion that it is by oxidating, and not by diſoxidating, that it produces its aſtoniſhing effects. Beſides, we have inſtances of ſubſtances which act in this manner. The oxigenated muriatic acid, far from abſtracting the oxigen, is

* The pieces of beech charcoal which I uſe, and which weigh thirty-ſix grains, are of the length of ten lines, and about five in diameter.

† See my third memoir on charcoal.

ſuppoſed

ſuppoſed, on the contrary, to add a conſiderable quantity of it to fetid ulcers, and carries off their fetidity as well as charcoal.*

Water may ſometimes be uſed in the Inſtrument inſtead of Mercury.

42d. Although, in the courſe of theſe experiments, I have conſtantly employed mercury, in ſome experiments a coloured water may be uſed with advantage, ſince it is better ſeen in the tube; but this cannot be uſed when the gaſes which are abſorbed by water are examined, nor with the carbonic acid and the other acid gaſes which are much abſorbed: then it is neceſſary to change the tube, and to ſubſtitute one which, inſtead of eighteen inches, is at leaſt thirty-ſix in height; without which it will be entirely filled.

Coloured water may ſometimes be ſubſtituted to mercury in the machine.

43d. It was on atmoſpheric air taken in my room with the windows open, that I experimented with coloured water. The charcoal produced an abſorption of ten inches and a half. It has been ſeen, that, with the mercurial apparatus, I had had an abſorption of eight inches ſome lines.

The preſſure of the atmoſphere in the water therefore augmented the abſorption, which uſually takes place with the mercury, by two inches and a half; that is to ſay, one-fourth of the height.

The preſſure of the atmoſphere affects the reſults.

44th. I ſhould obſerve, that very frequently the ſmall differences of one or two lines obſerved in the abſorptions of the ſame quality of air or of gas, are owing to the variations of the atmoſphere, which I aſcertained by comparing the height of the barometer during the experiment.

The Pieces of Charcoal uſed in theſe Experiments, acquire Weight.

45th. After the experiments, all the pieces of charcoal employed had acquired greater weight: this augmentation was from half a grain to two grains, and appears to be dependent on the greater abſorption.

The charcoal acquires weight in theſe experiments;

46th. I took four pieces of charcoal which had been uſed in the experiments made with atmoſpheric air, which, among them, had acquired four grains in weight: I paſſed them

* See the notes of M. Odier to Beddoes's memoir. Bibl. Brit. Vol. VI. p. 359.

under water, in a tube of one inch in diameter and ſix inches high, which was alſo filled with water. A part of the air which they contained was immediately developed, but I only obtained about two inches, (they had, however, abſorbed among them nearly thirty-two inches). The reſtored air was found to be compoſed of carbonic acid gas and azote gas. In this operation a part of the air is fixed in the charcoal to be embodied with it, and cannot be again diſlodged but by employing fire. I believe theſe combinations frequently occur in nature, and the great Newton ſuſpected it.

and yields a portion of the abſorbed air, which is a compound of azote and carbonic acid gaſes;

47th. I dried the pieces of charcoal which had been wetted in the preceding experiment, in the air: I afterwards examined them, by again heating them to redneſs in the fire, and placing them in the machine; they abſorbed the ſame quantity of air as before.

by which it regains its abſorbent power.

48th. I directed all my attention, as well after as during the courſe of the experiments on atmoſpheric air, to ſee if I could perceive any drops of water either in the tube or in the apparatus, but I did not obſerve the leaſt trace of it; neither was the charcoal moiſt; on the contrary, its ſurface was covered with aſhes.

No water produced by the diminution of the gas under examination.

Experiments to be made by ſaturating the Charcoal with different Subſtances.

49th. Since, during the progreſs of an inveſtigation, there are always new experiments which offer themſelves, I perceived that it would be intereſting to make trial with pieces of charcoal ſaturated with different ſaline and acid ſubſtances, as M. M. Rouppe and Van-Norden, of Rotterdam, had undertaken to do, and to ſee the effects they would produce, with my machine, as well in atmoſpheric air as in the different gaſes: But my memoir being of ſufficient length already, I leave the care of theſe enquiries to other philoſophers; and this the more willingly, ſince it appears that one of the moſt able chemiſts, M. Van Mons, has engaged in this ſubject, which, in his hands, will doubtleſs receive every degree of perfection which can be deſired.

Experiments ſtill to be made.

50th. I ſhall only give the reſult of four experiments which I made with atmoſpheric air.

I ſaturated pieces of charcoal with nitric acid, ſulphuric acid, ſolution of potaſh, and lime-water; they were not uſed until

until they had been thoroughly dried on filtering-paper. I put them cold into my machine. The following are the results:

1ſt. In the firſt ſix hours the charcoal, ſaturated with nitric acid, produced an abſorption of four lines and a half; afterwards it gave back the air, and even depreſſed the mercury below its level. Effects of charcoal ſaturated with nitric acid;

2d. The charcoal, ſaturated with ſulphuric acid, effected an abſorption of nine lines, which it preſerved uniformly for twenty-four hours. with ſulphuric acid;

3d. The charcoal, ſaturated with ſolution of potaſh, made an abſorption of three lines, and remained ſo for twenty-four hours. with ſolution of potaſh; and

4th. That, ſaturated with lime-water, made only a ſmall abſorption of a line and a half, although it was kept in experiment for twenty-four hours. with lime-water.

Theſe four experiments prove that charcoal, ſaturated with different ſubſtances, loſes, in part, its power of abſorbing: probably theſe ſolutions, by filling the pores of the charcoal, in ſome meaſure deprive it of this property. Charcoal loſes its abſorbent power by being ſaturated with ſome ſubſtances.

5ſt. No perſon can be more aware than I am, that my enquiry is far from being carried to that degree of perfection which I wiſhed: I hope, nevertheleſs, that philoſophers will not be diſſatisfied that I have preſented them with new experiments, and have made them acquainted with an inſtrument, by means of which they can appreciate the effects of ſolar light and caloric on charcoal; an inſtrument which, in many caſes, will ſerve them for an eudiometer, and which, when azote ſhall be better known, will acquire greater preciſion. I hope that, by the labours of other philoſophers who may employ themſelves on this ſubject, this intereſting branch of natural philoſophy will be brought to perfection, and an explanation of theſe ſurpriſing phenomena be obtained.

VI.

Farther Obſervations on the Conſtitution of mixed Gaſes. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

An apology for this letter.

OTHER philoſophers, beſides Mr. Dalton, have maintained the exiſtence of free elaſtic vapour in the atmoſphere; but as the modification given by this gentleman to the opinion, is perhaps the laſt it will receive, I am deſirous of treſpaſſing once more on your pages, in reply to my opponent, as well as to ſhew the changes which the preſent controverſy has produced in his ſyſtem.

Some objections ſtated oy Mr. D. conſidered.

Mr. Dalton's laſt letter begins by expoſing my ignorance of chemiſtry, and diſapproving the data, by help of which air has been ſhewn to be a chemical compound. Although his ſuſpicions may be juſt in many inſtances, they are wrong in this; for the facts ſtated by Mr. D. were known to me at the time of writing that paper; and the reprobated data were adopted for the following reaſons:

Specific gravity of oxigenous gas.

Mr. Davy found the weight of 100 cubic inches of oxigenous gas, of the temperature of 50°, to be 35.06 grains, the barometer ſtanding at 30 inches: now the denſities of the ſame elaſtic fluid at 50° and 60°, are in the ratio of 105824 to 103744*: but when the magnitude is given, *i. e.* 100 parts, the weights are as the denſities; conſequently the weight of 100 cubic inches of oxigenous gas at 60°, is 34.37 grains, the barometer ſtanding at 30: but 31 grains being the weight of an equal bulk of air in like circumſtances, and its ſpecific gravity being denoted by 1000, the correſponding ſpecific gravity of oxigenous gas is expreſſed by the number 1108; which exceeds Mr. Kirwan's expreſſion only by five. This approximation to an exact coincidence, affords the ſtrongeſt evidence of the correctneſs both of Mr. Kirwan and Mr. Davy, that can be expected in a queſtion of the kind; and, as the former gentleman made uſe of the ſame apparatus to determine the weight of air which had loſt its oxigen by being expoſed to

* Thomſon's Chemiſtry, Vol. I. p. 342.

the

the martial paſte of ſulphur, the ſpecific gravity, drawn from his experiment, was certainly to be preferred in my calculation to any other. This reſiduum was called by me azote, probably becauſe Mr. Kirwan called it phlogiſticated air: but it was evidently conſidered as a compound in the eſſay alluded to by Mr. Dalton; and certainly the advocate for a multiplicity of co-exiſtent atmoſpheres will not controvert the ſuppoſition. The remaining objections made by my opponent to the paper in queſtion, are levelled at my ignorance, not at my concluſions; they ſhall therefore be paſſed over in ſilence. In the mean time, Mr. D. perhaps will not take it amiſs if I requeſt him to repeat his calculations with the ſpecific gravity of oxigenous gas when properly corrected, and to try what will be the variation of the eudiometer, either on the data of Mr. Davy or M. Lavoiſier.

A new inſtrument recommended.

Permit me, Mr. Nicholſon, to conclude this part of my ſubject by obſerving, that an inſtrument of great uſe in pneumatics might be conſtructed on the principles of your hydroſtatic weighing machine. The body of it ſhould be of glaſs, furniſhed with proper ſtopples and ſtop-cocks, ſo that it might be eaſily exhauſted and repleniſhed. The preceding is but an imperfect ſketch of an inſtrument, the mechaniſm of which I reſign to your pen and pencil, ſhould the idea appear worth purſuing.

A miſrepreſentation pointed out.

Mr. Dalton's ſtrictures on my laſt letter aſcribe to me very ſingular opinions; but he neglects to point out the paragraph where the grounds of this charge may be found; a charge which inſinuates, that, according to my notions of things, a veſſel containing a cubic foot of one gas cannot receive an equal bulk of a different kind: Here indeed he accuſes me of groſs ignorance; for there are but few who do not know that fermenting liquors increaſe the denſity of the gaſes, which occupy part of the cloſe veſſels containing them. In reality, the charge is ſo perfectly groundleſs, that, had it been made by almoſt any other perſon, I would not have heſitated to have pronounced my accuſer guilty of miſrepreſentation to conceal his want of argument, and to ſecure the prejudices of the ſuperficial reader in his own favour. He may court the admiration of ſuch; I do not deſire their applauſes.

Mr. D.'s important argument no argument at all.

Mr. Dalton ſpeaks with confidence of what he calls his important argument, *i. e.* his aſſertion, that equal quantities of vapour

vapour enter a vacuum and an equal ſpace previouſly occupied by a gas. No one would refuſe its due importance to this argument, after ſeeing or peruſing a ſatisfactory demonſtration; but I know of no ſuch thing in Mr. Dalton's eſſays. He aſſerts, indeed, that a quantity of dry air, confined in a tube, receives an augmentation of force from the preſence of water, equal to that which is produced by the ſame cauſe in the exhauſted tube. But this is no demonſtration, unleſs the poroſity of air be firſt admitted; for, without this conceſſion, Mr. D. has no right to eſtimate the quantity of vapour preſent by the ſpring of it; this ought to be determined by weight only. But when did Mr. D. weigh an exhauſted veſſel, and afterwards take its weight when filled with vapour? When has he repeated the ſame experiment upon the ſame veſſel containing dry and moiſt air, and found the reſult favourable to his hypotheſis? Negative anſwers to theſe queſtions muſt convince an impartial judge, that the poroſity of the air has been again artfully introduced to eſtabliſh its own exiſtence: thus we find the fundamental datum of the ſyſtem to be demonſtrated by arguing in a circle, which begins and ends with the thing to be proved.

On the other hand, Mr. D. is convinced that the admiſſion of vapour into an open bottle expels part of the air. This is a proof that ſteam, ſuppoſing its exiſtence in the atmoſphere, finds a ſpace crowded with gaſes leſs convenient than a vacuum; it, therefore, makes room for itſelf, by diminiſhing the denſities of theſe gaſes, and creates in them Mr. Dalton's capacity for the reception of water. This fact being eſtabliſhed, it will alſo be evident, that, when vapour is forced into a gas confined in a cloſe veſſel, the former cannot make its way through the latter, otherwiſe than by condenſing it: Such is the nature of the pores of the air, according to Mr. D.'s own principles. The atmoſphere is alſo found to oppoſe a ſimilar reſiſtance to newly developed gaſes; for the gaſeous compound contained in an open veſſel filled in part with a fermenting liquor, is not a column of the atmoſphere permeated by an additional body of the carbonic acid, but a mixture, in which the proportion of common air is comparatively ſmall. This fact overturns Mr. D.'s general concluſion, that atmoſpheres may be added to the compound atmoſphere at pleaſure,

ſure, or taken from it, without its diſturbing the denſities and ſituations of the remaining atmoſpheres*.

The free paſſage of gaſes through each other was once the fundamental maxim of my opponent; at which time he maintained, that the variations in the weight of the atmoſpheric compound ariſe from the changeable nature of the aqueous atmoſphere, not from the permanent gaſes†. This dogma is eſſential to the ſyſtem; it has, however, been abandoned by its author himſelf; for Mr. D. when attempting to explain the obſervation of Mr. Kirwan, is obliged to admit the permanent gaſes to be more abundant in dry than in moiſt air; that is, the denſities and poſitions of theſe gaſes depend upon the ſtate of the contained vapour, contrary to the hypotheſis. This departure from the primitive maxim is avowed openly in Mr. D.'s explanation of the experiment with the moiſt bottle. Here he is compelled to confeſs, that ſteam, at low temperatures, diſtends the pores of air; and endeavours to preſerve the exiſtence of his aqueous atmoſphere by ſomething, which looks like a demonſtration from the final Q. E. D.: but it ought to be remembered, that the particles of a fluid preſs equally in all directions; conſequently, that the corpuſcles of air would act with their full force on the contiguous corpuſcles of his ſuppoſed vapour. Change in Mr. D.'s opinions.

Perhaps Mr. D. will perceive, by this time, that logical preciſion has placed him in the circumſtances of certain philoſophers mentioned in his third eſſay, who are unable to defend their opinions. In fact, Mr. D.'s hypotheſis is repugnant to natural appearances in its primitive form; for, according to it, a ſhower of rain is a prodigy; ſeeing the drops muſt diſplace equal bulks of air, and this removal muſt be brought about by the inadequate weight of a column of vapour which has loſt part of its ſpring.

JOHN GOUGH.

Middleſhaw, December 13, 1804.

P. S. Since the publication of my paper on vegetation, I have found, by Dr. Thomſon's Chemiſtry, that M. Ingenhouz had formerly made the ſame diſcovery. In 1795, I read

* Mancheſter Memoirs, Vol. V. p. 546.

† Mancheſter Memoirs, Vol. V. p. 547.

his

his work, *Sur les Vegetaux*, printed at Paris, 1787; but undoubtedly this ſection had by ſome means been overlooked. All that I have to do at preſent, by way of reparation, is to thank Dr. Thomſon for his information, and to declare that paper of no value; which in all probability will prove a ſufficient apology to M. Ingenhouz.

VII.

On two Metals, found in the black Powder remaining after the Solution of Platina. By SMITHSON TENNANT, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

Introduction, with reference to the experiments made by Deſcotils,

UPON making ſome experiments, laſt ſummer, on the black powder which remains after the ſolution of platina, I obſerved that it did not, as was generally believed, conſiſt chiefly of plumbago, but contained ſome unknown metallic ingredients. Intending to repeat my experiments with more attention during the winter, I mentioned the reſult of them to Sir Joſeph Banks, together with my intention of communicating to the Royal Society, my examination of this ſubſtance, as ſoon as it ſhould appear in any degree ſatisfactory. Two memoirs were afterwards publiſhed in France, on the ſame ſubject; one of them by M. Deſcotils, and the others by Meſſrs. Vauquelin and Fourcroy. M. Deſcotils chiefly directs his attention to the effects produced by this ſubſtance on the ſolutions of platina. He remarks, that a ſmall portion of it is always taken up by nitro-muriatic acid, during its action on platina; and, principally from the obſervations he is thence enabled to make, he infers, that it contains a new metal, which, among other properties, has that of giving a deep red colour to the precipitates of platina.

and by Vauquelin.

M. Vauquelin attempted a more direct analyſis of the ſubſtance, and obtained from it the ſame metal as that diſcovered by M. Deſcotils. But neither of theſe chemiſts have obſerved, that it contains alſo another metal, different from any hitherto known.

Subſtance obtained from the grains of platina,

The ſubſtance with which my experiments were made, was obtained from platina which had been previouſly freed from the ſand and other impurities generally mixed with it; ſo that

it

it muſt have been contained in the ſubſtance of the grains of platina. Though it has ſomewhat the appearance of plumbago, it may eaſily be diſtinguiſhed by its ſuperior weight. By weighing it in a phial with water, I found its ſpecific gravity almoſt 10.7.

reſembling plumbago, but heavier.

Before I deſcribe the method of ſeparating the two metals of which it conſiſts, it may be worth while to mention the effects of it, when combined with different metals in its entire ſtate. It readily unites with lead; but, even with ten times its own weight, the compound has not, when melted, much fluidity. Upon diſſolving the lead in nitrous acid, the black powder was obtained, with little apparent alteration, not having been entirely broken down, but conſiſting chiefly of the ſame ſcaly particles as at firſt. With biſmuth, zinc, and tin, the effects were nearly ſimilar; but, by fuſion with copper in a very ſtrong heat, a more perfect union was produced. On attempting to diſſolve the compound by nitro-muriatic acid, me of the powder was taken up with the copper, forming a very dark ſolution.

It combines with metals:—A ſmall portion renders lead, biſmuth, zinc, and tin, difficultly fuſible with copper;

The undiſſolved portion conſiſted partly of the ſubſtance in its original form of ſcales, and partly of a blacker powder, the particles of which were too ſmall to be viſible, and which had probably been completely combined with the copper. This ſubſtance may be eaſily united, by fuſion, with ſilver or gold; and it is particularly deſerving of attention, that it cannot be ſeparated from theſe metals, by the uſual proceſs of refining. It remains combined with either of them, after cupellation with lead; and with the gold, after quartation with ſilver. The alloys retain conſiderable ductility; and the colour of that with gold, is not materially different from pure gold.

ſilver or gold; not ſeparable by refining.

I ſhall now proceed to deſcribe the analyſis of the black powder, and the properties of the two metals which enter into its compoſition. The method which I uſed for diſſolving it, was ſimilar to that employed by M. Vauquelin, the alternate action of cauſtic alkali, and of an acid. I put a quantity of the powder into a crucible of ſilver, with a large proportion of pure dry ſoda, and kept it in a red heat for ſome time. The alkali being then diſſolved in water, had acquired a deep orange, or browniſh-yellow colour, but much of the powder remained undiſſolved. This powder, digeſted in marine acid, gave a dark blue ſolution, which afterwards became of a duſky

Analyſis of the black powder; which conſiſts of two metals.

Fuſion with ſoda.

Solution in water, deep orange, leaving a powder; which was partly diſſolved in muriatic acid.

duſky olive-green, and finally, by continuing the heat, of a deep red colour. Part of the powder being yet undiſſolved by the marine acid, was heated as before with alkali; and, by the alternate action of the alkali and acid, the whole appeared capable of ſolution. At each operation, ſome ſilex was taken up by the alkali; and, as this continued till the metallic part was entirely diſſolved, it ſeems to have been chemically combined with it.

Fuſion, &c. repeated on the undiſſolved reſidue.

Silex.

The alkaline ſolution contains the oxide of a volatile metal, not yet noticed, but which I ſhall preſently deſcribe, and alſo a ſmall proportion of the other metal. If this ſolution is kept for ſome weeks, the latter metal ſeparates ſpontaneouſly from it, in the form of very thin flakes, of a dark colour.

The alk. ſolution contains an oxide which ſeparates by repoſe.

The acid ſolution alſo contains both the metals, but principally that which has been mentioned by the French chemiſts. The properties of this laſt metal, which they have remarked, are thoſe of giving a red colour to the triple ſalt of platina with ſal-ammoniac, of not being altered by muriate of tin, and of giving, with pure alkali, a dark brown precipitate. M. Vauquelin alſo adds, that it is precipitated by galls, and by pruſſiate of potaſh; but I ſhould rather aſcribe theſe precipitates to ſome impurity, and probably to iron.

The acid ſol. contains the ſame metal, but principally that examined by Vauquelin.

As it is neceſſary to give ſome name to bodies which have not been known before, and moſt convenient to indicate by it ſome characteriſtic property, I ſhould incline to call this metal *Iridium*, from the ſtriking variety of colours which it gives, while diſſolving in marine acid.

The name *Iridium* given to the metal examined by Vauquelin.

In order to obtain the compound of this metal with marine acid in a pure ſtate, I tried to make it cryſtallize.

By ſlow evaporation of the ſolution, only an imperfectly cryſtallized maſs was produced; but this, being dried on blotting-paper, and diſſolved in water, afforded, by again evaporating as before, diſtinct octaedral cryſtals. Theſe cryſtals diſſolved in water, gave a deep red coloured ſolution, inclining to orange. With an infuſion of galls no precipitate was formed, but the colour was inſtantly, and almoſt intirely, taken away. Muriate of tin, carbonate of ſoda, and pruſſiate of potaſh, produced nearly the ſame effect. Pure ammonia precipitates the oxide; but (poſſibly from adding it in exceſs) I found it retained a part in ſolution, acquiring a purple colour. The pure fixed alkalis alſo precipitate the greater part of the

Cryſtals of its muriate diſſolved in water.

Precip. by alkalis and metals.

oxide, but are capable of retaining a part in ſolution, becoming of a yellow colour. All the metals which I tried, excepting gold and platina, produced a dark or black precipitate from the muriated ſolution, which is at the ſame time deprived of its colour. The iridium may be obtained in a pure ſtate, merely by expoſing the octaedral cryſtals to heat, which expels the oxigen and the muriatic acid. It appeared of a white colour, and was not capable of being melted, by any degree of heat I could apply. I could not combine it with ſulphur, nor with arſenic. Lead eaſily unites with it; but is ſeparated by cupellation, leaving the iridium upon the cupel, as a coarſe black powder. Copper forms with it a very malleable alloy, which, after cupellation with the addition of lead, left a ſmall proportion of the iridium, but much leſs than in the former caſe. Silver may be united with it, and the compound remains perfectly malleable. The iridium was not ſeparated from it by cupellation, but occaſioned on the ſurface a dark or tarniſhed hue. It appeared not to be perfectly combined with the ſilver, but merely diffuſed through the ſubſtance of it, in the ſtate of a fine powder. Gold alloyed with iridium is not freed from it by cupellation, nor by quartation with ſilver. The compound was malleable; and did not differ much in colour from pure gold, though the proportion of alloy was very conſiderable. If the gold or ſilver is diſſolved, the iridium is left, in the form of a black powder.

The metal iridium is white, infuſible, and rejects ſulphur and arſenic.

Its habitudes with the metals.

The yellow alkaline ſolution, which I have already mentioned as containing a metallic oxide, diſtinct from the former, is conſidered by M. Vauquelin as a ſolution of the oxide of chrome in alkali; but I could not, by any teſt, diſcover the preſence of chrome. After the ſuperfluous alkali had been neutralized by an acid, it produced a pale or buff-coloured precipitate with a ſolution of lead, and not the bright yellow which is given by chrome. But, as we are indebted to the above diſtinguiſhed chemiſt, among many other important diſcoveries, for our knowledge of the exiſtence of chrome, it is not improbable that ſome kinds of platina may contain that ſubſtance, beſides the other bodies uſually mixed with it. When the alkaline ſolution is firſt formed, by adding water to the dry alkaline maſs in the crucible, a pungent and peculiar ſmell is immediately perceived. This ſmell, as I afterwards

The alkaline ſolution did not contain *Chrome*.

It contains the volatile oxide of a metal called *Oſmium*;

wards diſcovered, ariſes from the extrication of a very volatile metallic oxide; and, as this ſmell is one of its moſt diſtinguiſhing characters, I ſhould on that account incline to call the metal *Oſmium*.

expellable from the alkali by an acid;

This oxide may be expelled from the alkali by any acid, and obtained in ſolution with water by diſtillation. The ſulphuric acid, being the leaſt volatile, is the moſt proper for this purpoſe; but as, even of this acid, a little is liable to paſs over, a ſecond ſlow diſtillation is required, to obtain the oxide perfectly free from it. The ſolution thus procured is without colour, has a ſweetiſh taſte, and the ſtrong ſmell before mentioned. Paper ſtained blue with violets, was not changed by it to red; but, by being expoſed to the vapour of it in a phial, the paper loſt much of its blue colour, and inclined to gray. As a certain quantity of this oxide is extricated during the ſolution of the iridium in marine acid, that part may alſo be obtained by diſtillation.

or obtained by diſtilling the black powder with nitre.

Another mode by which the oxide of oſmium may be obtained in ſmall quantity, but in a more concentrated ſtate, is, by diſtilling with nitre the original black powder procured from platina.

Proceſs deſcribed.

With a degree of heat hardly red, there ſublimes into the neck of the retort, a fluid apparently oily, but which, on cooling, concretes into a ſolid, colourleſs, ſemitranſparent maſs. This, being diſſolved in water, forms a ſolution ſimilar to that before deſcribed. The oxide, in this concentrated ſtate, ſtains the ſkin of a dark colour, which cannot be effaced.

Moſt ſtriking character of ſolution of oſmium:

The moſt ſtriking teſt of the oxide of oſmium, is an infuſion of galls, which preſently produces a purple colour, becoming ſoon after of a deep vivid blue. By this means, the preſence of this, and of the metal firſt deſcribed, may be obſerved, when the two are mixed together.

Habitudes with various bodies;

The ſolution of iridium is not apparently altered by being mixed with the oxide of oſmium; but, on adding an infuſion of galls, the red colour of the firſt is inſtantly taken away, and ſoon after the purple and blue colour of the latter appears. The ſolution of the oxide of oſmium with pure ammonia, becomes ſomewhat yellow, and ſlightly ſo with carbonate of ſoda. It is not affected by pure magneſia, nor by chalk; but with lime a ſolution is formed, of a bright yellow colour. The ſolution with lime gives with galls a deep red precipitate, which becomes

becomes blue by acids. It produces no effect on a ſolution of platina or gold; but precipitates lead of a yellowiſh-brown, mercury of a white, and muriate of tin of a brown colour.

The oxide of oſmium becomes of a dark colour with alcohol, and after ſome time, ſeparates in the form of black films, leaving the alcohol without colour. The ſame effect is produced by ether, and much more quickly. — alcohol;

This oxide appears to part with its oxygen to all the metals, excepting gold and platina. Silver being kept in a ſolution of it for ſome time acquires a black colour; but does not entirely deprive it of ſmell. Copper, tin, zinc, and phoſphorus, quickly produce a black or gray powder, and deprive the ſolution of all ſmell, and of the power of turning galls of a blue colour. This black powder, which conſiſts of the oſmium in a metallic ſtate and the oxide of the metal employed to precipitate it, may be diſſolved in nitro-muriatic acid, and then becomes blue with infuſion of galls. — the metals.

If the pure oxide of oſmium, diſſolved in water, is ſhaken with mercury, it very ſoon loſes its ſmell; and the metal, combining with the mercury, forms a perfect amalgam. Amalgam with mercury.

Much of the mercury may be ſeparated by ſqueezing it through leather, which retains the amalgam of a firmer conſiſtence. The remaining mercury being diſtilled off, a powder is left, of a dark gray or blue colour, which is the oſmium in its pure ſtate. By expoſing it to heat with acceſs of air, it evaporates, with the uſual ſmell; but, if the oxidation is carefully prevented, it does not ſeem in any degree volatile. Being ſubjected to a ſtrong white heat, in a cavity made in a piece of charcoal, it was not melted, nor did it undergo any apparent alteration. Heated in a ſimilar ſituation with copper and with gold, it melted with each of theſe metals, forming alloys which were quite malleable. Theſe compounds were eaſily diſſolved in nitro-muriatic acid, and, by diſtillation, afforded the oxide of oſmium with the uſual properties. Pure oſmium had by diſtilling off the mercury. It is gray, and not fuſible; nor volatile, unleſs oxided. Alloys.

The pure metal which has been previouſly heated, does not ſeem to be acted on by acids; at leaſt I could not perceive any effect produced by boiling it for ſome time with nitro-muriatic acid. By heating it in a ſilver cup with cauſtic alkali, Pure oſmium not ſoluble in acids; but eaſily with alkali.

alkali, it immediately combined with the alkali, and, with water, gave a yellow ſolution, ſimilar to that from which it was procured. Acids expel from this ſolution the oxide of oſmium, which has the uſual ſmell, and the power of giving to infuſion of galls the blue colour before mentioned.

VIII.

Remarks upon certain Obſervations of Mr. WILKINSON, *reſpecting Galvaniſm. By* RA. THICKNESSE, *Eſq.*

To Mr. NICHOLSON.

SIR, *Wigan, Dec.* 17, 1804.

Obſervations reſpecting galvaniſm, &c.

THE remarks of Mr. Wilkinſon (which I have not had an earlier opportunity of attending to) on my letter of the 20th September, on galvaniſm, are in general founded only on miſconception; the conſequence, probably, of a haſty peruſal in a public room. He attributes to me an aſſertion, "that two metals are requiſite to the production of galvanic phenomena;" but my words are, and particularly marked by italics, "two metals, *or other ſubſtances*;" and were intended to ſignify two metals, or two other ſubſtances; or one metal and one other ſubſtance; and to include even that which may be diſſolved in the water. The experiment, therefore, of La Grave, with brain and muſcle, which Mr. W. adduces, is not an exception; and "that a ſingle metal ſuffices" perfectly with pure water only, he did not, I fancy, wiſh to be underſtood.

That copper and the other negative metals are, as I expreſſed, "acted upon by the hidrogen," I muſt refer to Mr. W. for authority, to pages 177 and 178 of your Journal; and that particularly ſilver (my words are copper, or ſilver) is rendered more brittle, to page 85: but there cannot, ſurely, be ſtronger evidence that copper has an affinity for hidrogen, and conſequently an influence in the decompoſition of the water, than is afforded by the experiments in which hidrogen gas is obtained from the copper of the pile, and oxigen gas from the oxidated zinc; part of the hidrogen, and of the oxigen, being prevented from uniting with, and borne away from the metals by the electricity.

Mr.

Mr. W. imputes to me alſo the miſtake of having "obſerved that the ſenſation is in proportion to the ſurface acted on;" adding, that "the experiments of the French philoſophers proved, that the action of galvaniſm on animal ſubſtances is in the ratio of the number of plates employed, and not the ſurfaces expoſed." This is another inſtance of the inaccuracy of Mr. W.'s obſervations; for ſenſation I never mentioned; nor action on animal ſubſtances; not at all conſidering the intenſity, but the quantity only of the electric fluid produced; which may not always correſpond; for the charge of ſmall plates being weak, although according to their ſurfaces, the electricity meets with almoſt inſurmountable obſtructions from the interſtices of the pile; which to a ſtronger charge, from larger plates, are very trifling impediments: but Mr. W. thinks, himſelf, that "the production of galvanic phenomena is always proportionate to the degree of oxidation;" and muſt not the quantity of metal oxidated be in proportion to the ſurface expoſed to the water? The French philoſophers, too, have "concluded (I quote Mr. W.'s own words, from p. 207 of your laſt volume) that the effects of a galvanic battery on metallic ſubſtances, are in proportion to the ſurfaces of the plates employed:" Muſt we not then ſuſpect the experiments to have been imperfect which led them to conclude, if they have ſo concluded, that the effects on animal ſubſtances are different?

Obſervations reſpecting galvaniſm, &c.

Whether I "ſet out too hypothetically as to electricity being a modification of caloric," is a queſtion on which many of the moſt celebrated men of ſcience would differ from Mr. W. in opinion, "*Pars invenit utraque Cauſſas;* yet it appears to me, that the experiment noticed by *a Correſpondent*, p. 173, almoſt determines it: "If the gas which is produced from one of the wires communicating with the pile in the water, be united and inflamed with the other in a juſt proportion, the water which is common to both is reproduced, and *common fire* in great abundance." Thus the electric fluid, which alone conveys the oxigen and hidrogen from the water, actually becomes, when differently modified by combuſtion, for there is "no evidence of the preſence of fire until this point of time," active caloric.

But ſuppoſing, with Mr. W. that the electric matter and caloric are perfectly diſtinct bodies, we have ſtill the ſame

reaſon

Obſervations reſpecting galvaniſm, &c.

reaſon to conclude that the former is combined with water, which we have to believe that it is contained in metals: What then becomes of it when the water is decompoſed, if, as he imagines, the electric, or "galvanic phenomena, entirely depend upon the metal undergoing a chemical change," I muſt confeſs I cannot conjecture: but, in truth, it is entirely the cauſe of all the phenomena which take place: the quantity afforded by the metal, ſo very ſmall a portion of which undergoes any chemical change, muſt be too trifling to be conſidered; for if any more than the oxidated part of the metal contributed, the quantity of the metal in the plates, as well as the extent of its ſurface, would in ſome degree have an effect: but this, as I alledged, is not the caſe; and, therefore, Mr. W.'s fanciful conjecture, that there is as it were a ſpring of electric fluid in the metal, as a reſource for a ſupply, although very ingenious, may be regarded as a perfect hypotheſis—a ſuppoſition unſupported by any fact; and as an inſtance, from his partiality for it on this occaſion, of our diſpoſition to overlook imperfections in our own performances, whilſt we obſerve them in the works of others.

I am, Sir,

Your moſt obedient ſervant,

RA. THICKNESSE.

IX.

Abſtract of a Memoir on the Poſſibility of obtaining Pruſſiate of Potaſh free from Iron; the Unalterability of the Pruſſic Acid at high Temperatures; and the true Nature of the Combinations of this Acid with different Baſes. *By* BUCHOLZ.

(Concluded from p. 282, Vol. IX.)

General deductions.

THESE two experiments therefore prove, that from four parts of dried blood and one of carbonate of potaſh, no more pruſſiated alkali can be obtained than from two of blood and one of alkali.

From what has been ſo far ſtated the author concludes, after varying and repeating the experiments here detailed,

1. That

1. That four parts of blood to one of carbonate of potaſh are too much, and that equal parts of blood and carbonate of potaſh are rather good proportions for obtaining the moſt poſſible quantity of pruſſiated alkali. The latter proportions have, moreover, the diſadvantage, that the exceſs of alkalis acts upon the oxide of iron of the blood as well as upon the crucible, and therefore introduces new impurities. Two parts of blood and one of carbonate of potaſh, appear to be a better proportion for obtaining the greateſt poſſible quantity of pruſſiate of potaſh, under like circumſtances.

Beſt proportion of carbonate of potaſh and dried blood, are 1 p. and 2 b.

2. Alcalies deprived of carbonic acid, are not better than carbonated alkalies for the production of pruſſiated alkalies by treatment with blood; for they become reconverted into carbonated alkalies during the proceſs.

It is no advantage to uſe a pure alkali.

3. No pure pruſſiate of potaſh can be obtained, except care be taken that the mixture of blood and alkali be neither too much nor too little ignited. The criterion which may ſerve as a guide to the operator, is the ceſſation of the flame, after a gradually increaſed ignition without fuſion. For if the maſs be heated ſuddenly ſo as to effect a partial fuſion, or if it be continued after the diſappearance of the flame, it will be in vain to look for pure pruſſiated alkali.

The formation of pure pruſſiate requires a gradual heat without fuſion, and no longer than till the flame ceaſes.

4. The high colour of pruſſiated alkalis is not always owing to the preſence of iron, but more frequently to a minute quantity of charcoal. This impurity is not capable of yielding reſults that could miſlead the chemical enquirer in the application of this re-agent.

When the high colour of p. alk. ariſes from charcoal, it does not produce fallacy.

5. The quantity of water employed for the lixiviation of pruſſiated alkali from the ignited maſs, ſhould be as ſmall as poſſible, and heat ſhould be avoided as much as poſſible. For if pruſſiated alkalis be heated in contact with water, part of the pruſſic acid is liberated, ammonia is produced, and carbonate of potaſh formed.

Very little water ſhould be uſed, and heat avoided.

6. Acetic acid cannot be employed for removing the admixture of carbonate of potaſh from pruſſiated alkalis; for the energy of the affinity of pruſſic acid is leſs than the energy of the affinity of carbonic acid to potaſh; the union of the pruſſic acid and potaſh is therefore demoliſhed, in preference to that of the carbonate of potaſh.

Acetic acid cannot be uſed to take off the redundant potaſh.

X.

On a new Metal, found in Crude Platina. By WILLIAM HYDE WOLLASTON, *M. D. F. R. S. From the Philoſophical Tranſactions,* 1804.

Introduction.

NOTWITHSTANDING I was aware that M. Deſcotils had aſcribed the red colour of certain precipitates and ſalts of platina, to the preſence of a new metal; and although Mr. Tennant had obligingly communicated to me his diſcovery of the ſame ſubſtance, as well as of a ſecond new metal, in the ſhining powder that remains undiſſolved from the ore of platina; yet I was led to ſuppoſe that the more ſoluble parts of this mineral might be deſerving of further examination, as the fluid which remains after the precipitation of platina by ſal ammoniac, preſents appearances which I could not aſcribe to either of thoſe bodies, or to any other known ſubſtance.

New metal, *Rhodium.*

My inquiries having terminated more ſucceſsfully than I had expected, I deſign in the preſent Memoir to prove the exiſtence, and to examine the properties, of another metal, hitherto unknown, which may not improperly be diſtinguiſhed by the name of *Rhodium*, from the roſe-colour of a dilute ſolution of the ſalts containing it.

Palladium found in ore of platina.

I ſhall alſo take the ſame opportunity of ſtating the reſult of various experiments, which have convinced me, that the metallic ſubſtance which was laſt year offered for ſale by the name of Palladium, is contained (though in very ſmall proportion) in the ore of platina.

Remaining ſolution after precipitation of platina, contains iron and other metals.

The colour of the ſolution that remains after the precipitation of platina, varies, not only according to its ſtate of dilution, but alſo according to the ſtrength and proportions of the nitric and muriatic acids employed. This colour, though principally owing to the quantity of iron contained in it, ariſes alſo in part from a ſmall quantity of the ammoniaco-muriate of platina, that neceſſarily remains diſſolved, and from other metals contained in ſtill ſmaller proportions.

Precipitable by zinc or iron.

(A. 1.) To recover the remaining platina, as well as to ſeparate the other metals that are preſent from the iron, I have in ſome experiments employed zinc, in others iron, for their precipitation. The former appears preferable; but, when the latter has been uſed, the precipitate may immediately be freed from

from the iron that adheres to it, by muriatic acid, without the loſs of any of thoſe metals which are at preſent the ſubject of inquiry.

(A 2.) Having in one inſtance diſſolved ſuch a precipitate in nitro-muriatic acid, and precipitated the platina by ſal ammoniac, I ſuffered the remaining fluid to evaporate without heat; and obtained a mixture of various cryſtals, very different from each other in form and colour. From theſe, I ſelected for examination ſome that were of a deep red colour, partly in thin plates adhering to the ſides of the veſſel, and partly in the form of ſquare priſms having a rectangular termination.

The ſame reſidual ſolution affords cryſtals by ſpontaneous evap.

(A 3.) A portion of theſe cryſtals being heated in a ſmall tube, yielded ſal ammoniac by ſublimation, and left a black reſiduum, which, by greater heat, acquired a brilliant metallic whiteneſs, but could not be fuſed under the blowpipe. Having obtained this ſubſtance from a diſtinctly cryſtallized ſalt, I was inclined to conſider it as a ſimple metal; and, as I found it to be wholly inſoluble in nitro-muriatic acid, I judged it not to be platina.

Deep red cryſtals left a white metal by heat, —not platina.

(A 4.) The cryſtals alſo, inſtead of being nearly inſoluble, like the ammoniaco-muriate of platina, were diſſolved in a ſmall quantity of water, and gave a roſe-coloured ſolution. Upon mixing this with a ſolution of platina, the ammonia was tranſferred by ſuperior affinity to the latter, forming an ammoniaco-muriate of platina; and the precipitate was of a yellow colour. Conſequently, the metal contained in the ſalt, was neither platina nor that which gives the red colour to the ſalts of platina.

The cryſtals were not of platina, nor that which makes the ſalts of platina red.

It would be uſeleſs to detail my firſt unſucceſsful experiments made upon the properties of this metal, in hopes of diſcovering means by which its ſeparation from platina might be effected; I ſhall therefore confine myſelf to the following proceſs, which appears to be the moſt direct for procuring rhodium in a ſtate of purity. In the ſame proceſs alſo palladium is obtained, ſo as to afford a preſumption, that it is rather a natural ſimple body, than any artificial compound.

The baſe was the metal rhodium now firſt noticed.

(B 1.) Since the platina to be procured in this country, generally contains ſmall ſcales of gold intermixed, as well as a portion of the mercury which the Spaniards employ for the ſeparation of the gold, the platina uſed for my experiments, after being by mechanical means freed, as far as poſſible, from

Proceſs for obtaining it. Purification of the platina-grains.

all viſible impurities, was expoſed to a red heat, for the purpoſe of expelling the mercury. It was then digeſted for ſome time in a ſmall quantity of dilute nitro-muriatic acid, and frequently ſhaken, till the whole of the gold was diſſolved, together with any impurities that might ſuperficially adhere to the grains of platina.

Solution of the ore in nitro-mur. acid.

(B 2.) Of the ore thus prepared, nearly $2\frac{1}{2}$ ounces were then diſſolved in nitro-muriatic acid, (diluted for the purpoſe of leaving as much as poſſible of the ſhining powder,) and the whole ſuffered to remain in a moderate ſand heat, till completely ſaturated.

Precipitation by ſal amm.

(B 3.) Such a portion of this ſolution was then taken for analyſis, as correſponded to 1000 grains of the prepared ore. An ounce of ſal ammoniac was next diſſolved in hot water, and uſed for the precipitation of the platina. The precipitate obtained was of a yellow colour, and, upon being heated, yielded 815 grains of purified platina.

Reſidual liquor precipitated by zinc.

(B 4.) The water uſed for waſhing this precipitate having been added to the ſolution poured from it, a piece of clean zinc was immerſed in it, and ſuffered to remain, till there appeared to be no further action upon the zinc. The iron contained in the ore (to the amount of 14 or 15 *per cent.*) remained in ſolution. The other metals had ſubſided, in the form of a black powder, which I eſtimated between 40 and 50 grains; but, as there was no occaſion to weigh it with accuracy, I thought it better not to dry this precipitate, for, if it be heated, the rhodium is in danger of being rendered inſoluble.

Precipitate deprived of copper and lead by nitric acid; and then diſſolved (moſt part) in nitro-m. acid.

(B 5.) As I had previouſly aſcertained that this precipitate would contain platina, rhodium, the ſubſtance called palladium, copper, and lead, the two laſt metals were firſt diſſolved in very dilute nitric acid, aided by a gentle heat. The remainder, after being waſhed, was digeſted in dilute nitro-muriatic acid, which diſſolved the greater part, but left as much as $4\frac{1}{2}$ grains undiſſolved.*

Addition of common ſalt and evap. left ſoda-muriates of platina, palladium and rhodium. All but the triple ſalt of rhodium was waſhed off by alcohol.

(B 6.) To the ſolution were added 20 grains of common ſalt; and, when the whole had been evaporated to dryneſs with a very gentle heat, the reſiduum, which I had found, from prior

* It was preſumed that this reſiduum conſiſted principally of the metal called by Mr. Tennant Iridium; but, as it was accidentally miſlaid, and was not examined, it might alſo contain a portion of rhodium.

experi-

experiments, would conſiſt of the ſoda-muriates of platina, of palladium, and of rhodium, was waſhed repeatedly with ſmall quantities of alcohol, till it came off nearly colourleſs. There remained a triple ſalt of rhodium, which by theſe means is freed from all metallic impurities.

(C 1.) This ſalt, having been diſſolved in a ſmall quantity of hot water, and let to ſtand 12 hours, formed rhomboidal cryſtals, of which the acute angle was about 75°. Solution in water.

(C 2.) It was then again diſſolved in water, and divided into two equal portions. Of theſe, one was decompoſed by a piece of zinc, and the other examined by the following re-agents. One half precip. by zinc.

(C 3.) Sal ammoniac occaſioned no precipitation; but, when a ſolution of platina was added to the mixture, a precipitate was immediately formed, and the colour of this precipitate was yellow; which again proves that the metal contained in this ſalt, is neither platina itſelf nor that which gives the red colour to its precipitates. The other half was precip. by platina, but not by ſal-amm.

(C 4.) Pruſſiate of potaſh occaſioned no precipitation, as it would have done, if the ſolution had contained palladium. nor by pr. potaſh,

(C 5.) Hydro-ſulphuret of ammonia, which would have precipitated either platina or palladium, cauſed no precipitation of this metal. nor by hydro-ſulph. of amm.

(C 6.) The carbonates of potaſh, of ſoda, or of ammonia, occaſioned no precipitation; but the pure alkalis precipitated a yellow oxide, ſoluble by exceſs of alkali, and alſo ſoluble in every acid that I have tried. nor by carb. alkalis; but by pure alk. ſoluble in acids.

(D 1.) The ſolution of this oxide in muriatic acid, upon being evaporated, did not cryſtallize; the reſiduum was ſoluble in alcohol, and of a roſe colour. Sal ammoniac, nitre, or common ſalt, cauſed no precipitation from the muriatic ſolution; but formed triple ſalts, which were not ſoluble in alcohol. Muriatic ſolution of rhodium.

(D 2.) The ſolution in nitric acid alſo did not cryſtallize. A drop of this ſolution, being placed upon pure ſilver, occaſioned no ſtain. On the ſurface of mercury a metallic film was precipitated, but did not appear to amalgamate. The metal was alſo precipitated by copper and other metals, as might be preſumed, from the uſual order of their affinities for acids. Nitric ſolution.

(E 1.) The precipitate obtained by zinc (C 2.) from the remaining half of the ſalt, appeared in the form of a black powder, weighing, when thoroughly dried, nearly 2 grains, correſponding to about 4 grains in the 1000 of ore diſſolved. The precip. before made by zinc was black,

(E 2.)

ſimply heated,

(E 2.) When expoſed to heat, this powder continued black; with borax, it acquired a white metallic luſtre, but appeared infuſible by any degree of heat.

fuſible with arſenic and with ſulphur.

(E 3.) With arſenic, however, it is, like platina, rendered fuſible; and, like palladium, it may alſo be fuſed by means of ſulphur. The arſenic, or the ſulphur, may be expelled from it by a continuance of the heat; but the metallic button obtained does not become malleable, as either of the preceding metals would be rendered by ſimilar treatment.

Metallic button not malleable,

unites with metals except mercury.

(E 4.) It unites readily with all metals that have been tried, excepting mercury; and, with gold or ſilver it forms very malleable alloys, that are not oxidated by a high degree of heat, but become incruſted with a black oxide, when very ſlowly cooled.

Gold and rhodium 4 to 1.

(E 5.) When 4 parts of gold are united with one of rhodium, although the alloy may aſſume a rounded form under the blowpipe, yet it ſeems to be more in the ſtate of an amalgam than in complete fuſion.

Gold and rhodium 6 to one.

(E 6.) When ſix parts of gold are alloyed with one of rhodium, the compound may be perfectly fuſed, but requires far more heat than fine gold. There is no circumſtance in which rhodium differs more from platina, than in the colour of this alloy, which might be taken for fine gold, by any one who is not very much accuſtomed to diſcriminate the different qualities of gold. On the contrary, the colour of an alloy containing the ſame proportion of platina, differs but little from that of platina. This was originally obſerved by Dr. Lewis. "The "colour was ſtill ſo dull and pale, that the compound (five "to one) could ſcarcely be judged by the eye to contain any gold."*

It does not diſcolour gold.

I find that palladium reſembles platina, in this property of deſtroying the colour of a large quantity of gold. When one part of palladium is united to ſix of gold, the alloy is nearly white.

Solubility of rhodium affected by combination.

(E 7.) When I endeavoured to diſſolve an alloy of ſilver or of gold with rhodium, the rhodium remained untouched by either nitric or nitro-muriatic acids; and, when rhodium had been fuſed with arſenic or with ſulphur, or when merely heated by itſelf, it was reduced to the ſame ſtate of inſolubility. But,

* Lewis's Philoſophical Commerce of Arts, p. 526.

when

when one part of rhodium had been fuſed with three parts of biſmuth, of copper, or of lead, each of theſe alloys could be diſſolved completely, in a mixture of two parts, by meaſure, of muriatic acid, with one of nitric. With the two former metals, the proportion of the acids to each other ſeemed not to be of ſo much conſequence as with lead; but the lead appeared on another account preferable, as it was moſt eaſily ſeparated, when reduced to an inſoluble muriate by evaporation. The muriate of rhodium had then the ſame colour and properties, as when formed from the yellow oxide precipitated from the original ſalt. (D 1.)

(E 8.) The ſpecific gravity of rhodium, as far as could be aſcertained by trial on ſo ſmall quantities, ſeemed to exceed 11. That of an alloy conſiſting of one part rhodium and about two parts lead, was 11,3; which is ſo nearly that of lead itſelf, that each part of this compound may be conſidered as having about the ſame ſpecific gravity. Specific gravity.

F. As it was expected that the alcohol employed for waſhing the ſalt of rhodium (B 6.) would contain the ſoda-muriates of platina and of palladium, the platina was firſt precipitated by ſal ammoniac. This precipitate was of a deep red colour; and, when it had been heated, to expel the ſal ammoniac, the platina which remained was of a dark gray colour. The alcohol before uſed in waſhing the ſalts examined.

(G 1.) To the remaining ſolution, after it had been diluted to prevent any further precipitation of platina, I added pruſſiate of potaſh, which inſtantly occaſioned a very copious precipitate, of a deep orange-colour at firſt, but changing afterwards to a dirty bottle-green, which I aſcribed to iron contained in the pruſſiate.

(G 2.) This precipitate, when dry, weighed $12\frac{1}{2}$ grains. After it had been heated, it left a metallic reſiduum, in ſmall grains, of a gray colour, weighing nearly 7 grains. A ſmall portion of it being heated with borax, communicated a dark brown colour to the borax, as from iron, and acquired a bright metallic luſtre, but could not be fuſed under the blowpipe. With ſulphur, however, it fuſed immediately into a round globule, which, by floating upon mercury, appeared of leſs ſpecific gravity than that metal.

(G 3.) The whole quantity was then treated in the ſame manner, and purified by cupellation with borax, till it cooled with a bright ſurface. From the globule the ſulphur was expelled,

pelled, by expoſure to the extremity of the flame; and it became ſpongy and malleable, weighing in this ſtate very nearly five grains.

Palladium.

(G 4.) A portion of this metal was diſſolved in ſtrong nitrous acid, was precipitated by green ſulphate of iron, and in other reſpects, poſſeſſed all the properties aſcribed to the palladium offered for ſale, in the printed paper that accompanied it, as well as others ſince noticed by Mr. Chenevix.

Its differences from platina.

(G 5.) In its precipitation by pruſſiate, it differs moſt eſſentially from platina; and conſequently is by no means difficult to be diſtinguiſhed, or ſeparated from it.

(G. 6.) The action of muriate of tin upon the ſolutions of theſe metals, is alſo totally different. A dilute ſolution of platina, is thereby changed from a pale yellow to a tranſparent blood-red. A ſolution of palladium, on the contrary, uſually becomes opaque, by the formation of a brown or black precipitate; but, if mixed in ſuch proportion as to remain tranſparent, it changes to a beautiful emerald-green.

(G 7.) In the formation of triple ſalts with the alkalis, as obſerved by Mr. Chenevix, palladium may be ſaid to reſemble platina; but the ſalts thus formed are far more ſoluble than the correſponding ſalts of platina, and differ entirely, in the colour and form of the cryſtals.

(G 8.) The ſoda-muriate of palladium is a deliqueſcent ſalt; that of platina, on the contrary, forms permanent cryſtals.

(G 9.) The triple ſalts of platina, with either muriate of ammonia or of potaſh, form octaedral cryſtals of a yellow colour, that are very ſparingly ſoluble in water. The correſponding ſalts of palladium, likewiſe reſemble each other in every reſpect. The cryſtals are very ſoluble in water, but inſoluble in alcohol; their form is that of a four-ſided priſm, and they each preſent a curious contraſt of colour, that certainly is not obſervable in any known ſalt of platina.

Curious contraſt of colour in the ſalts of palladium.

(G 10.) Although the ſolution is of a deep red, the cryſtals are of a bright green when viewed tranſverſely. In the direction of their axes, however, the colour is the ſame as that o the ſolution; but, on account of its extreme intenſity, it is with difficulty diſtinguiſhed in fragments that exceed $\frac{1}{100}$ of an inch in thickneſs. One conſequence of this colour is nevertheleſs

very

very obſervable; namely, that in viewing any cryſtal obliquely, it appears of a dull brown, that ariſes from a mixture of the red and green.*

The characters of palladium that have been enumerated, undoubtedly belong to none of the ſimple ſubſtances that we are acquainted with; and no experiment that I have made, has tended to confirm the ſuſpicion of its being a compound, conſiſting of any known ingredients. The experiments above related, ſhow evidently, that the ore of platina contains a very ſmall quantity of palladium; and it is not unlikely that this may have been a conſtituent part of ſome of the compounds obtained by Mr. Chenevix, and may have miſled him, by ſome properties which he would conſequently obſerve, into the ſuppoſition that he had formed palladium.

Probability that Mr. Chenevix was miſled to infer that palladium is a compound, from its being found in platina.

It is not, however, without having repeatedly endeavoured to imitate his experiments, that I have ventured to diſſent from ſuch authority. I made many attempts to unite pure platina with mercury, by ſolution, and by amalgamation; but without ſucceſs, in any one inſtance.

All attempts of the author to compare it were unſucceſsful.

From a ſolution of platina, carefully neutralized, as Mr. Chenevix directs, with red oxide of mercury, and mixed with a ſolution of green ſulphate of iron, I indeed obtained ſuch a

* The change of colour above deſcribed, though certainly uncommon, is nevertheleſs not peculiar to the ſalts of palladium, but may be ſeen alſo in ſome kinds of tourmalin. Among thoſe which come to us from Ceylon, ſome are tranſparent; and one variety is of a deep red in the direction of its axis, but of a yellowiſh green when viewed tranſverſely. There is alſo a correſponding, but oppoſite contraſt of colours, that has been obſerved by Muller, and deſcribed by Bergmann, in ſome of the Tyroleſe tourmalins. The general aſpect of theſe ſtones is black, and apparently opaque.—Some, however, of which the fracture is vitreous, are found to tranſmit a yellowiſh-red light when viewed tranſverſely, but in the direction of their axis the colour is a dull bottle green.

In each of theſe tourmalins, as well as in the ſalts of palladium, the colour in the direction of the axis is at leaſt ten times more intenſe than in the tranſverſe direction. A thin lamina, cut from the end of a Tyroleſe tourmalin for this purpoſe, tranſmitted no viſible light, till it was reduced to $\frac{1}{60}$ of an inch in thickneſs; and, when leſs than $\frac{1}{100}$ of an inch, it was not more tranſparent than another portion of the ſame cryſtal ſeen tranſverſely, $\frac{1}{10}$ of an inch in thickneſs.

precipitate

precipitate of metallic flakes as he deſcribes; but, upon examination of theſe flakes, they yielded mercury by diſtillation; and the remainder conſiſted of platina combined with a portion of iron, but had not any properties which I could ſuppoſe owing to the preſence of palladium.

Other facts and obſervations to ſhow that palladium is a ſimple metal.

Upon comparing the ſpecific gravity of this ſubſtance, which was ſaid to be, at moſt, 11,8, with that of mercury or of platina, I was always ſtrongly inclined to doubt the poſſibility of its being compoſed of theſe metals. I could recollect no one inſtance, in which the ſpecific gravity of any compound is leſs than that of its lighteſt ingredient, and could not, without careful examination, admit the ſuppoſition, that mercury could be rendered lighter by intimate union with platina. It now appears fully confirmed that this perſuaſion, ariſing from uniform experience, was well founded; for, if we conſider the difficulty of producing even an imperfect imitation of palladium, the failure of all attempts to reſolve it into any known metals, the facility of ſeparating it from any mixed ſolution of thoſe which it has been ſuppoſed to contain, as well as the number and diſtinctneſs of its characteriſtic properties, I think we muſt claſs it with thoſe bodies which we have moſt reaſon to conſider as ſimple metals.

XI.

Letter from Mr. Wm. Wilson, *exhibiting the Electricity of Metals, without the Help of any condenſing Inſtrument.*

To Mr. NICHOLSON.

SIR,

London, Dec. 22, 1801.

Whether the electricity from metals alone be the effect of the contact or the ſeparation,

WHEN I ſet about making the compound electrical condenſer deſcribed in my laſt letter to you, I intended to repeat the experiments of Cavallo relating to the electricity obtained by the contact of metals related in the third volume of the fourth edition of his Treatiſe of Electricity; but before the inſtrument was finiſhed, I was induced (by ſome experiments I had made relative to the cauſe of excitation of electricity) to ſuppoſe that it is not the contact of the metals that is the cauſe of the appearance of electrical ſigns, but the ſeparation of the metals

metals from contact. And this ſuppoſition was very much ſtrengthened, when, upon examination, I found that all who have made experiments on this ſubject have ſeparated the metals from contact before they examined them as to their electricity.

If the contact of the two metals be the cauſe of the electrical ſigns, the whole effect that one metal can have on another will be communicated at the time of contact, however few the points are that form the contact, becauſe both the metals being conductors of electricity, if one poſſeſſed a greater proportion of it than the other, a part will be communicated to the other at the time of contact, to form an equilibrium, and this will be done as well by a few points of contact as by a great many. But if it is the ſeparating them from contact, that is the cauſe of the electrical ſign, the more extenſive the contact is the more powerful will the ſigns be when the metals are ſeparated. —determinable by varying the ſurfaces.

To put this to the teſt of experiment, I pierced a piece of thin ſheet copper full of ſmall holes, juſt big enough to permit to paſs through them two or three particles of filings of another metal at a time, ſo that almoſt every particle muſt be in contact with the copper before it can paſs through, and conſequently the ſurface of contact be very great with a comparatively ſmall quantity of metal. Sieve of one metal and filings of another.

I ſifted through this copper ſieve ſome filings of zinc into a tin plate laid on the cap of a gold leaf electrometer, and the gold leaves diverged near an inch with poſitive electricity, when about half an ounce of filings had been ſifted into it. Encouraged by this ſtriking reſult, I procured ſieves and filings of different metals. The reſults of the trials with them are contained in the following table; where P ſtands for poſitive electricity, N for negative, and when it was not ſtrong enough to effect the electrometer, that is denoted by O.

Reſults of experiments.

Filings of	Sifted through	Effect.
Copper	Copper	O.
	Tin	N. ſtrong
	Silver	O.
	Zinc	P. ſtrong
	Lead	N.
Zinc	Copper	P. ſtrong
	Tin	O.
	Silver	P. ſtrong
	Zinc	P. exceeding weak
	Lead	O.

Results of experiments.

Filings of	Sifted through	Effect.
Tin	Copper	P. rather ſtrong
	Tin	N. very weak
	Silver	P. ſtrong
	Zinc	O.
	Lead	N. weak
Silver	Copper	O.
	Tin	N. weak
	Silver	O.
	Zinc	N.
Steel	Copper	N. very ſtrong
	Tin	N. ditto
	Silver	N. ditto
	Zinc	N. ditto
	Lead	N. ditto
Lead	Copper	P. very ſtrong
	Tin	O.
	Silver	P. very ſtrong
	Zinc	P. not quite ſo ſtrong as with ſilver
	Lead	O.
Braſs	Copper	P. very ſtrong
	Tin	N. ſtrong
	Silver	P. ſtrong
	Zinc	O.
	Lead	N.
Biſmuth	Copper	P. exceeding ſtrong
	Tin	O.
	Silver	P. exceeding ſtrong
	Zinc	P. ſtrong
	Lead	N.
Antimony	Copper	O.
	Tin	N. very ſtrong
	Silver	P. ſtrong
	Zinc	N. ſtrong
	Lead	N. very ſtrong
Nikel	Copper	P. exceeding ſtrong
	Tin	O.
	Silver	P. ſtrong
	Zinc	O.
	Lead	P.

In

In all the above experiments there was a large ſurface of contact, and the electrometer only was uſed; but in thoſe made by Cavallo and others who had a very ſmall ſurface of contact, electrical ſigns could not be made to appear without the help of doublers, multipliers, &c. of electricity. I therefore think there can be no doubt about the ſeparating the metals from contact being the cauſe of their appearing.

If you think the above worthy of a place in your Phlioſophical Journal, you will very much oblige me by inſerting it therein. I am your obedient humble ſervant,

WM. WILSON.

XII.

Analytical Experiments and Obſervations on Lac. By CHARLES HATCHETT, *Eſq. Partly abridged, but chiefly extracted from the Philoſophical Tranſactions for* 1804, *by* W. N.

IT is uncertain at what time the uſeful ſubſtance, called Lac, firſt became known in Europe. It has long been uſed by the Indians for purpoſes different from thoſe to which it is applied in Europe. In particular as Mr. Wilkins informed the author, they diſſolve ſhell lac in water by the addition of a little borax, and by adding lamp black or ivory black, they obtain an ink, not eaſily affected by water, when dry.

Uſes of lac peculiar to India.

Aqueous ſolution by borax affords an ink.

The natural hiſtory of lac, which is the nidus of the coccus or chermes lacca, has been detailed by Mr. Kerr, Mr. Saunders, and Dr. Roxburgh, in the Philoſophical Tranſactions for 1781, 1789, and 1791. It is diſtinguiſhed into four kinds, of which, however, only three are commonly known in commerce, *viz.* ſtick lac, ſeed lac, and ſhell lac; the difference of theſe, with that of the fourth, called lump lac, is as follows.

Natural hiſtory of lac.

Four kinds.

1. Stick lac, is the ſubſtance or comb in its natural ſtate, incruſting ſmall branches or twigs.

1. Stick lac.

2. Seed lac, is ſaid to be only the above, which has been ſeparated from the twigs, and reduced into ſmall fragments; but I ſuſpect it to have undergone ſome other proceſs, as I have found the beſt ſpecimens to be very conſiderably deprived of the colouring matter.*

2. Seed lac.

3. Lump

* Mr. Wilkins informs me that the crude lac, as it is taken from the branches and twigs of the trees, is uſually deprived of its colouring

3. Lump lac. 3. Lump lac, is formed from ſeed lac, liqueſied by fire, and formed into cakes. And,

4. Shell lac. 4. Shell lac, according to Mr. Kerr and Mr. Saunders, is prepared from the cells, liqueſied, ſtrained, and formed into thin tranſparent laminæ, in the following manner.

How made from the cells. "Separate the cells from the branches; break them into ſmall "pieces; throw them into a tub of water, for one day; waſh "off the red water; dry the cells, and with them fill a cylindri- "cal tube of cotton cloth, two feet long, and one or two inches "in diameter; tie both ends, and turn the bag above a charcoal "fire; as the lac liqueſies, twiſt the bag, and, when a ſufficient "quantity has tranſuded the pores of the cloth, lay it upon a "ſmooth junk of the plantain tree, and with a ſtrip of the plan- "tain leaf draw it into a thin lamella; take it off while flexible, "for in a minute it will be hard and brittle." †

The degree of preſſure on the plantain tree, regulates (according to Mr. Saunders) the thickneſs of the ſhell; and the quality of the bag determines its fineneſs and tranſparency.

Lac is moſtly had from Aſſam. Aſſam furniſhes the greateſt quantity of the whole of the lac now in uſe. ‡

Beſt quality. Mr. Kerr (ſpeaking of ſtick lac) obſerves, that the beſt lac is of a deep red colour; for, if it is pale and pierced at the top, the value is diminiſhed, becauſe the inſects have left their cells, and conſequently theſe can be of no uſe as a dye or colour, but probably may be better for varniſhes.

The ſeed lac which I have examined, contained but little of the colouring matter, and appeared (as I have already obſerved) to have undergone ſome proceſs of purification; but, of all the varieties, ſhell lac contains the leaſt of the tinging ſubſtance, as may well be expected, when the mode of preparing it is conſidered.

Chemiſts have paid little attention to lac. It is remarkable, that although lac has been known, and imported into Europe, during ſo long a time that the date cannot now be aſcertained, yet it has but little attracted the attention of chemiſts.

ing matter by boiling, having been previouſly reduced, by pounding into ſmall fragments. In Bengal, the ſilk dyers are the people who thus produce what we call the ſeed lac, which they do for the ſake of the colour.

† Phil. Trans. 1781, p. 378. ‡ Phil. Tranſ. 1789, p. 109.

The firſt chemiſt of eminence who mentions it, and the only one who has ſubjected it to any thing like a regular examination, is the younger Geoffroy, whoſe Paper is publiſhed in the *Mém. de l'Acad. de Paris* for the year 1714.* In this Paper, Mr. Geoffroy ſeems to have been chiefly induced to examine it on account of its tinging ſubſtance; but he nevertheleſs has not neglected the ſubſtance which conſtitutes the cells. This he conſiders to be a ſort of wax, very diſtinct from the nature of gum or reſin. But it is to be obſerved, that he formed this opinion, not ſo much upon the reſults of chemical experiments, as upon the cellular conſtruction obſerved in the ſtick lac, which, as he juſtly remarks, demonſtrates it to be formed by inſects, after the manner that the honeycomb is formed by bees; and that it is not therefore, as ſome have ſuppoſed, a gum or reſin, which has exuded from vegetables ſimply punctured by inſects.† Experiments of Geoffroy the younger;

Geoffroy and Lemery obtained from lac, by diſtillation, ſome acid liquor, and a butyraceous ſubſtance. Moreover, Geoffroy obſerves, that when ſtick lac was thus treated, ſome ammonia was alſo obtained, but not when ſeed lac was employed. —and Lemery.

He alſo mentions another ſort of lac, brought from Madagaſcar, and called by the natives *Lit-in-bitſic*. This ſubſtance, he ſays, is ſcarcely to be diſtinguiſhed from bees-wax, which it much reſembles in colour and odour; and that it is produced by a grayiſh inſect, much larger than the chermes lacca. It is evident however, from Geoffroy's deſcription, that this ſubſtance is very different from the common lac; and there can be little doubt, but that it is the ſame as that which was, a few years ago, examined by Dr. Pearſon, under the name of white lac, a ſubſtance reſembling the Pé-la of the Chineſe. ‡ Another lac which appears to have been the Pe-la of China.

Geoffroy (as I have ſtated) conſidered lac as a ſort of wax; and ſince his time it has ſcarcely, if at all, been ſubjected to Notions of chemiſts concerning lac.

* Obſervations ſur la Gomme Lacque, et ſur les autres Matières animales qui fourniſſent la Teinture de Pourpre. Par. M. Geoffroy le jeune. *Mém. de l'Acad.* 1714. p 121.

† Mr. Kerr obſerves, that as a red ſubſtance is obtained by inciſion from the plaſo tree, very analogous to lac, it is probable, that the inſects have little trouble in animalizing the ſap of theſe trees, in the formation of their cells. Phil. Trans. 1781, p. 377.

‡ Phil. Trans. 1794, p. 383.

chemical

chemical examination; it is not therefore ſurpriſing, that the opinions of chemiſts concerning it have been various. Chaptal adopts the opinion of Geoffroy, and calls it a kind of wax; * but Gren † and Fourcroy ‡ regard it as a true reſin.

§ I.

Chemical Experiments on Lac. Its Habitudes with Solvents.

Water extracts the colour from lac.

1. Water digeſted upon powdered ſtick lac becomes of a deep crimſon colour, and takes up about 10 per cent. Seed lac gives out no more than $2\frac{1}{2}$ or 3 per cent. and ſhell lac only $\frac{1}{2}$ per cent. by ſimilar treatment.

Alcohol diſſolves the reſin, which is a large part.

2. Alcohol diſſolves much of the lac, taking up reſin in a clear ſolution if cold; but if heated, the ſolution is turbid and ſcarcely to be rendered tranſparent, either by repoſe or filtration.

The reſin is obtained either by evaporating the ſolution or by pouring it into water acidulated with muriatic or acetic acid. The reſin which is thus precipitated may be ſeparated by the filter. It amounts to 67 or 68 per cent. in ſtick lac, and to 88 in the ſeed lac. Shell lac contains 81 per cent. beſides 10 per cent. which is defended from the ſpirit, and cannot be ſeparated but by ſubſequent operation. The reſin is leſs brittle than other reſin.

Vegetable gluten.

When the ſhell lac was in ſmall fragments only, theſe after the firſt ſeparation of the reſin, retained their figure and were more bulky, porous, and elaſtic. The elaſticity was deſtroyed by boiling water, and the matter itſelf proved to be of the nature of vegetable gluten.

Ether affects lac very little.

3. Sulphuric ether acts leſs powerfully on lac than alcohol does. It conſiderably ſoftens the lac; but produces little other effect.

Sulphuric acid converts it into coal.

4. Concentrated ſulphuric acid firſt acts on the colouring matter; but after a ſhort digeſtion on the ſand bath, the fluid acts on the whole maſs, and becomes firſt reddiſh brown, and afterwards black, ſulphureous acid gas being evolved, and the chief part of the lac being at length ſeparated in a ſtate reſembling coal.

* Chaptal's Elements; Engliſh edition. Vol. III. p. 387.

† Principles of modern Chemiſtry. Vol. I. p. 388.

‡ *Syſtême des Connoiſſances chimiques.* Tome V. p. 624.

Nitric

5. Nitric acid applied to lac emits nitrous gas at firſt, and cauſes the lac to ſwell much at the ſame time that it converts it into a deep opake yellow brittle ſubſtance, which by a ſufficiency of nitric acid, and continuance of the digeſtion for 48 hours, is diſſolved. The ſolution however is turbid, and when poured into a large quantity of diſtilled water, depoſits ſome yellowiſh flocculi, which, being collected, are found to be a ſort of wax.

Nitric acid by management and time diſſolves it.

The ſolution is turbid and by dilute depoſits a wax.

The filtrated liquor is of a bright golden yellow; and, when ſaturated by ammonia, changes to orange colour, but does not yield any precipitate, nor any traces of oxalic or malic acid.

The filtered ſolution contains a yellow matter.

This yellow nitric ſolution is converted, by evaporation, into a deep yellow ſubſtance, which burns like reſin, but is ſoluble in boiling water.

Its properties, &c.

The alkalis and lime, being added to this aqueous ſolution, do not produce any precipitate, but the yellow colour is very conſiderably deepened; and, by evaporation, an orange-coloured ſubſtance is obtained, which is ſtill eaſily ſoluble in water, and conſiſts of the deep yellow ſubſtance above-mentioned, combined with the alkali or lime.

6. Muriatic acid diſſolves the colouring matter and gluten of lac; but its action on theſe is feeble, unleſs the reſin has been previouſly ſeparated.

6. Muriatic acid diſſolves the colouring matter and gluten.

7. Acetous acid, in its effects, much reſembles muriatic acid.

7. Acetous acid.

8. Stick lac, ſeed lac, and ſhell lac, are partially diſſolved by acetic acid; and, if this be heated, a conſiderable portion is taken up.

8. Acetic acid acts upon the whole except the wax.

The diſſolved part conſiſts of the colouring extract, of reſin, and of gluten; the wax being the only ingredient which is inſoluble in this menſtruum; but a portion of the former ſubſtances, being enveloped by the wax, are protected from the action of the acetic acid.

The acetic ſolution of lac becomes turbid when cold, and depoſits part of the reſin; a portion however remains in ſolution, and may be precipitated by water; after which, the liquor retains ſome gluten and colouring extract, which may be precipitated by ſaturating the acid with an alkali, and by ſubſequent boiling.

For the reaſons above ſtated, it would be difficult to effect a complete ſolution of lac by means of acetic acid; but this may

 nevertheleſs

nevertheleſs be advantageouſly employed in analytical operations, when alternately uſed with alcohol.

Boracic acid ſcarcely acts on lac.

9. A ſaturated ſolution of boracic acid in water, diſſolves the colouring extract; but, as the effect does not ſurpaſs that of water alone, we may conclude that lac is little, if at all, acted upon by boracic acid.

Borax renders it ſoluble in water.

10. It has been already ſtated, that ſub-borate of ſoda or borax has a powerful effect on lac, ſo as to render it ſoluble in water; and, as the preceding experiments prove that boracic acid alone ſcarcely acts upon lac, there is every reaſon to believe, that the exceſs of ſoda preſent in borax is the active ſubſtance; and this concluſion will be confirmed, by the reſults of ſubſequent experiments made with the alkalis.

Proportion, &c.

In order to render lac (eſpecially ſhell lac) ſoluble in water, about $\frac{1}{5}$ of borax is neceſſary; and this may be previouſly diſſolved in the water, or may be mixed and added together with the lac.

The beſt proportion of water to that of lac is 18 or 20 to 1. So that 20 grs. of borax, and four ounces of water, are, upon an average, requiſite to diſſolve 100 grs. of ſhell lac; but more water may be occaſionally added, to ſupply the loſs cauſed by evaporation during the digeſtion, which ſhould be made nearly in a boiling heat.

This ſolution of ſhell lac is turbid, and of a reddiſh-brown colour; when conſiderably diluted with water and agitated, a weak lather is formed; it is decompoſed by acids, and the lac is precipitated in yellow flocculi, which do not apparently differ from the lac originally employed.

The ſolution is a ſoap of difficult ſolubility.

The general properties of the ſolution ſhew, that it is a ſaponaceous compound, which, being uſed as a varniſh or vehicle for colours, becomes (when dry) difficultly ſoluble in water, although this was the liquid employed to form the ſolution.

A white thick ſcum or cream collects on the ſurface of this liquid, after it has been ſuffered to remain tranquil for ſome time, and is found to be produced by a ſort of wax, which I ſhall more particularly notice when the analyſes of the varieties of lac are deſcribed; but, in the preſent caſe, this wax appeared in ſome degree to be converted into an almoſt inſoluble ſoap by the alkali of the borax, and may be regarded as the principal cauſe of the turbidneſs of the ſolution.

11. The

11. The lixivia of pure ſoda and of carbonate of ſoda completely diſſolve the different kinds of lac; and theſe ſolutions exactly reſemble thoſe formed by means of borax, excepting that they are deeper coloured. Soda and its carbonate diſſolve lac.

Rather leſs than $\frac{1}{5}$ of carbonate of ſoda is required to diſſolve ſhell lac; and this ſolution, when dried, is ſooner affected by damp or water than the ſolution prepared by borax.

12. Lixivium of pure or cauſtic potaſh ſpeedily diſſolves the varieties of lac, and forms ſaponaceous ſolutions, ſimilar to that in which borax was employed, excluſive of the colour, which is deeper, and more approaching to purple. Alſo potaſh,

Lixivium of carbonate of potaſh extracts a great part of the colouring matter, but does not form ſo complete a ſolution of the entire ſubſtance of lac, as when pure potaſh is employed. and its carbonate.

The above alkaline ſolutions, by repoſe, afford the waxen ſoap which has been mentioned; and acids, being added to theſe ſolutions, and to that formed by borax, precipitate the lac in a flocculent ſtate, and of a yellow or buff colour, which precipitate, when melted, becomes ſimilar to the lac originally employed. If however an alkaline ſolution of ſhell lac (prepared, for inſtance, with ſoda) be gradually dropped into a ſufficient quantity of muriatic acid diluted with an equal portion of water, and nearly heated to the boiling point, and if after boiling the whole for about one hour the coagulum be ſeparated, and the clear liquor be carefully ſaturated with ſoda, and again made to boil, a ſmall quantity of a flocculent precipitate is obtained, which was found to be analogous to precipitated vegetable gluten, combined with ſome of the colouring extract.

13. Pure ammonia, and carbonate of ammonia, readily act upon the colouring matter of lac, but do not completely diſſolve the entire ſubſtance. Ammonia acts partially.

§ II.

Analytical Experiments on Stick, Seed, and Shell Lac.

Lac, placed on a red hot iron, contracts, melts, ſmokes much, and leaves a ſpongy coal. Slow diſtillation carried to the red heat, gave from ſtick lac, 1. Water ſlightly acid, 10; Products of lac by deſtructive diſtillation. Stick lac.

2. Thick brown butyraceous oil, 59; 3. Spongy coal, 13½; 4. A ſmall portion of carbonate of ammonia, with carbonic acid, carbonated hidrogen, and hidrogen gas, by eſtimate 17½.

Seed lac. Seed lac, by the ſame proceſs, gave, 1. Acidulated water, 6; 2. Thick oil, 61; 3. Spongy coal, 7; 4. Mixed gas as before, but without ammonia, 26.

Shell lac. Shell lac gave, 1. Acidulated water, 6; 2. Oil, 65; 3. Spongy coal, 7½; 4. Mixed gas, 21½.

The coal of this laſt gave one grain of aſhes after combuſtion, which contained a muriate, probably of ſoda. There was alſo a little iron, and ſome particles of ſand, probably extraneous.

Analyſis of Stick Lack.

Powdered ſtick lac gave a red colouring extract to water. A. 200 grains of ſtick lac, picked and reduced to powder, were digeſted in a pint and a half of boiling diſtilled water during 12 hours. The liquor was tranſparent, and of a beautiful deep red; this was decanted into another veſſel; and the operation was repeated, with freſh portions of water, until it ceaſed to be tinged; the lac then appeared of a pale yellowiſh-brown colour.

The whole of the aqueous ſolution being evaporated, left a deep red ſubſtance, which poſſeſſed the general properties of vegetable extract, and weighed 18 grains.

Alcohol then took up much reſin. B. The dried lac was digeſted for 48 hours, without heat, in eighteen ounces of alcohol; and the clear tincture being cautiouſly decanted different portions of alcohol were added, and the digeſtion was repeated, until the alcohol ceaſed to produce any effect.

The whole of the ſolutions in alcohol were then poured into diſtilled water, which was heated, and an attempt was made to ſeparate the precipitated ſubſtance by filtration; but, as this did not ſucceed, on account of the filter ſpeedily becoming clogged, the whole was ſubjected to gentle diſtillation; by which, a browniſh-yellow reſin was obtained, amounting in weight to 136 grains.

C. The remainder of the lac was again digeſted in boiling diſtilled water; by which, 2 grains of the colouring extract were obtained.

Diluted muriatic acid took gluten from the remainder. D. The reſiduum was then digeſted with one ounce of muriatic acid diluted with two ounces of water, which, by boiling,

ing, became of a bright pale red, but changed to purple, when ſaturated with a ſolution of carbonate of potaſh.

A flocculent precipitate was thus obtained, which poſſeſſed the characters of precipitated vegetable gluten combined with ſome of the colouring extract; this, when completely dried, weighed 11 grains.

E. There now remained 25 grains, which evidently conſiſted of a ſort of wax, mixed with ſmall parts of twigs and other extraneous ſubſtances. The reſidue was wax and foreign matter.

A part of the wax was ſeparated by heat and preſſure in a piece of linen; and another portion was ſeparated by digeſtion in olive oil, which aſſumed the conſiſtency of an unguent.

The reſiduum was then boiled with lixivium of potaſh, and became tinged with purple, in conſequence of ſome of the colouring extract which had not been diſſolved by the preceding operations.

The undiſſolved part, now conſiſting only of the extraneous vegetable and other ſubſtances, weighed 13 grains; ſo that the wax, with a ſmall portion of the colouring extract, may be eſtimated at 12 grains.

By the above proceſs, 200 grains of ſtick lac afforded, Component parts of ſtick lac.

			Grs.
A. C.	Colouring extract - -	18 2 -	20
B.	Reſin - - - - -		136
D.	Vegetable gluten - - - -		11
E.	Wax, with a little colouring extract, about -		12
	Extraneous ſubſtances - - -		13
			192.

Analyſis of Seed Lac.

200 grains of very pure ſeed lac were ſubjected to operations very ſimilar to thoſe which have been deſcribed, and afforded, Component parts of ſeed lac.

	Grs.
Colouring extract - - -	5
Reſin - - - - -	177
Vegetable gluten - - -	4
Wax - - - - -	9
	195.

Analyſis

Analyſis of Shell Lac.

Shell lac boiled in water gave little extract.

A. 500 grains of this ſubſtance were firſt treated with boiling diſtilled water, as above-mentioned, and yielded of extract only 2.50 grains.

Reſidue gave much reſin to alcohol.

B. The 497.50 grains which remained, were then digeſted with different portions of cold alcohol, until this ceaſed to produce any effect; the reſin which was thus ſeparated, amounted to 403.50 grains.

C. As the ſhell lac had not been reduced into powder, but only into ſmall fragments, theſe were become white and elaſtic, and, when dry, were brittle, and of a pale brown colour; the whole then weighed 94 grains.

Muriatic acid then took up gluten.

D. Theſe 94 grains were digeſted in diluted muriatic acid; and the acid, being afterwards ſaturated with ſolution of carbonate of potaſh, afforded a flocculent precipitate, (reſembling that obtained from ſolutions of vegetable gluten,) which, when dry, weighed 5 grains.

Acetic acid, by long digeſtion, took up reſin and wax; the latter was thrown down by alcohol;

E. Alcohol acted but feebly on the reſiduum; it was therefore put into a matraſs, with three ounces of acetic acid, and was ſuffered to digeſt without heat during ſix days, the veſſel being at times gently ſhaken; the acid thus aſſumed a pale brown colour, and was very turbid. The whole was then added to half a pint of alcohol, and was digeſted in a ſand-bath; by which a browniſh tincture was formed, and at the ſame time a quantity of a whitiſh flocculent ſubſtance was depoſited, which, being collected, well waſhed with alcohol on a filter, and dried, weighed 20 grains.

This ſubſtance was white, light, and flaky, and, when rubbed by the nail, it became gloſſy, like wax; it alſo eaſily melted, was abſorbed by heated paper, and, when placed on a coal or hot iron, emitted a ſmoke, the odour of which very much reſembled that of wax, or rather ſpermaceti.

and then the reſin by water.

F. The ſolution formed by acetic acid and alcohol, being filtrated, was poured into diſtilled water, which immediately became milky; and, being heated, the greater part of the reſin which had been diſſolved aſſumed a curdy form, and was partly ſeparated by a filter, and partly by diſtilling off the liquor; this portion of reſin amounted to 51 grains.

Carbonate of potaſh threw down ſome reſin.

G. The filtrated liquor, from which this reſin had been ſeparated, was ſaturated with a ſolution of carbonate of potaſh;

potaſh; and, being heated, a ſecond precipitate of gluten was obtained, which, when well dried, weighed 9 grains.

The 500 grains of ſhell lac thus yielded,

Component parts of ſhell lac.

		Grs.
A.	Extract - - -	2.50
B. C.	Reſin - - - -	454.50
D. G.	Vegetable gluten - -	14.
E.	Wax - - - -	20.
		491.

The mode of analyſis adopted for the ſhell lac, muſt undoubtedly appear leſs ſimple than that which was employed for ſeed and ſtick lac: but, upon the whole, it was attended with advantages; for the ſhell lac being in ſmall fragments, and not in the ſtate of a powder, conſiderably facilitated the decantation of the ſolution in alcohol from the reſiduum; and although, in this laſt, a portion of the reſin was protected from the action of the alcohol, by being enveloped in the gluten and wax, yet, by the aſſiſtance of acetic acid, the remainder of the reſin, as well as the whole of the gluten, were diſſolved; the wax was obtained in a pure ſtate; and a ſeparation of the reſin from the gluten was afterwards eaſily effected, by the method which has been deſcribed. As therefore acetic acid is capable of diſſolving reſin, gluten, and many other of the vegetable principles, it certainly may be regarded as a very uſeful ſolvent, in the analyſis of bodies appertaining to the vegetable kingdom.

Advantages of the proceſs laſt uſed.

From the reſults of the preceding analyſes it appears, that the different kinds of lac conſiſt of four ſubſtances, namely, extract, reſin, gluten, and wax, the ſeparate properties of which ſhall now be more fully conſidered.

(The Concluſion in our next.)

Galvanic

XIII.

Galvanic Illuſtrations and Remarks. By C. WILKINSON, *Eſq.*

To Mr. NICHOLSON.

DEAR SIR, *Glaſgow, Dec. 9,* 1804.

On the galvanic charge, &c.

HAVING, during my galvanic tour, been occaſionally informed that the attempt I have made in my Elements of Galvaniſm, to explain the laws of galvaniſm on the more perfect and imperfect conductors, is not ſo clear as it was my wiſh to have rendered it; I ſhall therefore deem myſelf much obliged by the inſertion of the following obſervations in your valuable Journal.

The power of a galvanic apparatus, formed of plates not leſs than $2\frac{1}{4}$ inches in diameter, on animal ſubſtances, is directly as the number of plates; on the more perfect conductors, as metals, the power is as you have at large ſtated in your laſt Journal.

Let us ſuppoſe three galvanic plates: *Fig.* 1, *(Plate II.)* of four inches; *Fig.* 2, of ſix inches; and *Fig.* 3, of eight inches: An arrangement of theſe in the form of a galvanic trough, produces equal effects on animal ſubſtances, provided the number be equal.

Animal ſubſtance is very little ſuperior to water in its conducting power; and the experiments of Mr. Cavendiſh have proved, that the conducting powers of iron are a million times greater than the conducting powers of water.

A metal brought into contact with another metal diſpoſed to part with a quantity of its compound electricity, will divide the quantity thus diſengaged, becauſe its capacity for receiving it is equal to that of the metal under the chemical action.

Animal ſubſtance brought into contact with a metal ſimilarly diſpoſed, will only receive a portion adequate to its capacity; and as this capacity is in the ratio of its conducting powers, it will be nearly one million times inferior in capacity to that of a metallic ſubſtance; and will, therefore, abſtract only ſo ſmall a proportionate part of electricity to render its intenſity equal to that exiſting in the galvanic plate.

By electrical intenſity I underſtand the quantum of electricity multiplied by the reſiſting powers of the ſubſtance to admit it. Let us ſuppoſe the plates 1, 2, and 3, diſpoſed to give out electricity on every aſſignable point of their reſpective ſurfaces. A finger applied at No. 1, *Fig.* 1, will abſtract a certain portion of electricity from the points of metal directly in contact; a certain portion of electricity will immediately be determined from the ſquares 2, 3, 4, 5, 6, &c. from every correſponding ſquare on the ſurface of the plate, by that motion which Volta has well termed the moto-electrical power: there is a phyſical reſiſtance in the tranſmiſſion from theſe reſpective ſquares to the firſt touched by the finger, and the more remote the greater the reſiſtance: at whatever points this phyſical reſiſtance ſhall be equal to the capacity of the ſubſtance employed, there all further tranſmiſſion muſt ceaſe: this reſiſtance in plates of $2\frac{1}{4}$ inches in the ſquare, appears, from experiment, fully adequate to the conſtant ſupply of ſo imperfect a conductor as animal ſubſtances, and therefore any increaſe of ſize in the ſurface of the plate, will produce no additional effects.

When a certain quantity is thus abſtracted from the pair of plates at the extremes of the battery, in direct contact with the fingers, theſe plates and the interpoſed fluid in the cells, only act as conductors to the next pair, and ſo on; as the velocity of diſcharges of each ſucceſſive pair is ſo infinitely greater than the time required for the chemical action, is the reaſon why the effects we experience are increaſed in proportion to the numbers: if we were to ſuppoſe that the time required for the ſucceſſive diſcharge of 50 pair of plates, ſhould be preciſely equal to the time required for the chemical action, we ſhould derive no increaſe of power by an increaſe of number, which is contrary to experience.

I have a battery formed of 600 plates, each plate only expoſing half a ſquare inch in ſurface: To experience equal effects from this arrangement, I am obliged to pauſe three or four minutes between each contact; an interval of time no ways requiſite in larger plates.

When ſuch ſuperior conductors as metals are employed, the phyſical reſiſtance of tranſmiſſion from one part of the plate to another, bearing no proportion with the capacity of the

the metal, every aſſignable point of the galvanic plate is efficient at the ſame time, therefore the intenſity will be as the ſurface, and the powers as the ſquares of the intenſities.

I am, Dear Sir,

Your's, reſpectfully,

C. WILKINSON.

No. 19, *Soho Square.*

XIV.

On the Devitrification of Glaſs and the Phenomena which happen during its Cryſtallization. By DARTIGUES. *

Glaſs is not a cryſtallization.

SOME philoſophers have conſidered glaſs as a chryſtallization; and this opinion appears natural on conſidering the tranſparence of glaſs or of cryſtal, from the latter of which the term for deſignating the regular and ſpontaneous arrangement of the particles of bodies has been taken. But on cloſer reflection, this notion is found to be erroneous. In fact glaſs never affects a cryſtalline figure, either in its ſurface or its fracture; it never preſents chryſtals of its own proper ſubſtance, ſuch as are ſeen in metals ſlowly congealed; and whenever cryſtals are formed in the maſs of glaſs, they are foreign to the part which ſtill continues vitrified; they may be conſidered as a ſtep the reverſe from that of vitrification, as I ſhall ſhow in the courſe of the preſent paper.

Vitreous fuſion diſtinct from that of ſalts.

On the firſt explanation of vitreous fuſion, I muſt diſtinguiſh and ſeparate it from the fuſion of certain other bodies which flow without addition in the heat of our furnaces, ſuch as borax, the phoſphoric acid and others. In theſe bodies the con-

* Tranſlated from the Journal de Phyſique, LIX. 5.

The author of this memoir, who is proprietor of the glaſs-works and other eſtabliſhments of Voneche (Sambre-et Meuſe) has undertaken to draw up for the French National Inſtitute a treatiſe on the art of glaſs-making, to ſerve as a continuation to the arts and trades of the Academy. The firſt part is in readineſs to appear, many of the plates being ready. The ſecond part relates to the uſes of glaſs in the arts. And the third part conſiſts of detached memoirs upon the phyſical and chemical properties of glaſs. The preſent diſſertation is extracted from one of thoſe memoirs.—J. C. Delametherie.

denſed

denſed heat ſoftens and renders them fluid, and they more or leſs continue to exhibit, when cooled, the tranſparency and ſome other of the well known phyſical properties of glaſs.

It is a ſolution by heat.

But in the examination and deſcription of that fuſion to which vitreous compounds are ſubjected in order to fit them for the purpoſes of life, it is neceſſary to obſerve that the effect ariſes from two phenomena. It is not ſimply the reſult of accumulated heat, but it is jointly produced by the affinities of the ſubſtances which enter into the mixture. Theſe ſubſtances which tend to combine and mutually to penetrate each other, act according to the laws of their affinities as ſoon as ever the temperature is ſufficiently elevated. Thus it is that a mixture of ſeveral earths becomes fuſed at a temperature which could not have rendered any one of them fluid.

Nature of the compound; or glaſs.

The common vitrification is therefore the reſult of a combination effected at an elevated temperature between different and heterogenous ſubſtances; and the product is a compound perfectly homogenous more or leſs tranſparent, elaſtic, and breaking in a peculiar manner, from which the term vitreous fracture is derived. This body is a remarkably bad conductor of heat and of electricity; it is capable of becoming ſoft at a temperature inferior to that at which it was fuſed, ſo that it may be rendered paſty, ductile, &c.

Devitrification.

The phenomenon during which all theſe properties diſappear is what I have called devitrification; an expreſſion which may at firſt ſeem extraordinary, but which the facts will ſhew to be perfectly accurate.

It is the conſequence of cryſtallization.

Several philoſophers have before noticed this effect; ſome have even made obſervations upon it, and have noted various circumſtances ſcarcely connected with each other; but I am not aware that any one has yet publiſhed a ſeries of reſearches proper to explain the effects, ſo as to ſhew that it is one of the known properties of all natural bodies, being in fact nothing more than the product of a cryſtallization.

The porcelain of Reaumur:

Reaumur firſt obſerved that a glaſs, more eſpecially if it be compoſed of different earths, as bottle glaſs is in general, may be decompoſed, and loſe its tranſparency and other vitreous properties. Fully occupied in his reſearches upon porcelains, he was deſirous of applying this diſcovery to the fabrication of potteries, and attributed the phenomenon to the ſubſtances in which he had cemented his glaſs. The fact was

denominated the cementation of glaſs, and the product was called Reaumur's porcelain: no incident could be more effectually calculated to retard the true developement of the fact than ſuch a denomination of this kind.

Repetitions of the proceſs by d'Antic and others.

The labours of Boſc d'Antic upon the ſame object were alſo directed to no other purpoſe than that of obtaining a good pottery by this means, and of aſcertaining what cements were the beſt calculated to give new properties to this body. Thus it was that by calling the proceſs by the name of cementation, which depended in no reſpect upon what was added as the cement, others were miſled, who were induced to follow the courſe of experiments already began. The natural conſequence was, that ſcience gained nothing reſpecting this proceſs ſince the time of Reaumur. Many have ſince attempted the cementation of glaſs without perceiving any new reſult in the product.

Obſervation of cryſtals in glaſs.

Several perſons have ſince admitted the property in glaſs of affording cryſtals; but theſe remarks being more eſpecially made by artiſts, placed by their ſituation at the head of glaſs-works, have not afforded the ſcientific conſequences which might have been deduced from them. The directors of a great eſtabliſhment have ſeldom time to dwell upon the contemplation of ſmall effects; they are obliged to attend to too many things at the ſame time. Theſe remarks, though curious in themſelves, remained without connection; and no one thought or ventured to publiſh that the cryſtallization of glaſs, and cementation by the proceſs of Reaumur are abſolutely one and the ſame thing.

Experiments of Sir James Hall on whinſtone, &c.

Sir James Hall,* in his valuable experiments upon whinſtone and lava, publiſhed in the 14th volume of the Bibliotheque Britannique, aſcertained the property of theſe ſtones to become fuſed into glaſs, and to return to the ſtony ſtate, according to the circumſtances.

He did not purſue this devitrification among artificial products.

He called this laſt fact a devitrification. He ſaw that it was the effect of a precipitation, and explained it in a manner that was highly ſatisfactory and true; but being too much occupied in deducing from this fact arguments in favour of the volcanic geology, he neglected purſuing the intereſting philoſophical conſequences to which the phenomenon pointed the way. This is

* Firſt in the Edinburgh Tranſactions, and afterwards in our Journal. N.

the taſk I have undertaken, and in the preſent memoir on devitrification, I ſhall give the reſults of my firſt experiments. As my ſituation gives me the uſe of a fire of extreme violence and continued for years together, it may have been in my power to make obſervations not within the reach of every one. The facts I ſhall report do partly explain themſelves; they are the reſult of laws to which all bodies are ſubjected. All the merit of the obſervation conſiſts in having ſeen them in ſubſtances, and on occaſions where it was not known that theſe laws do take place.

It is very remarkably ſeen in glaſs furnaces.

The bottom of the furnaces for fonding or fuſing the glaſs uſually preſents large cavities excavated by the action of the fire, and of the corroſive ſubſtances which often flow out of the melting pots. Theſe cavities are filled with a kind of glaſs called picadil, which is compoſed of aſhes which become vitrified, ſtones of the furnace which become fuſed, and more particularly of glaſs which falls from the pots. Care is taken to remove this at each font or making of glaſs. When the furnace is nearly worn out, the cavities having become large, cannot be entirely cleanſed, but ſome of the picadil remains. When the furnace is extinguiſhed, this picadil undergoes an extremely ſlow cooling, becauſe it is ſurrounded with maſonry of ſeveral cubic fathoms, which is itſelf penetrated with heat that has continued for upwards of a year. I have always remarked that it was in the glaſs of theſe bottoms of the furnace that I found cryſtallizations through the maſs of glaſs, the reſt of which was very tranſparent and pure. Theſe cryſtallizations, which were always conſiderably regular and numerous, excited my curioſity, as they had done that of other glaſs-makers before me. I collected many ſpecimens, taking care to chuſe thoſe which appeared the moſt curious, and preſented the moſt extrordinary characters.

Cryſtals in glaſs.

Examination of the facts.

Soon afterwards, from comparing the pieces themſelves, and the circumſtances under which they had been produced; and by the combinations of remarks, trials, and experiments to imitate theſe cryſtallizations at pleaſure, I at laſt ſucceeded in diſtinguiſhing various claſſes, all of which are produced by the nature of the different circumſtances which accompany the formation of the glaſs. I ſhall give a rapid outline of theſe, without entering into any remarks on the devitrification which almoſt conſtantly takes place in the ſcoria of forge furnaces.

This

This laſt fact muſt have preſented itſelf to every one, and will without difficulty be referred to the obſervations I ſhall make.

The more compounded the glaſs the more eaſily it loſes its vitreous nature.

The firſt remark which offers itſelf is, that the more compounded the nature of the glaſs, the more eaſily and readily it will become devitrified: but in the ſame manner as a ſolvent loaded with a great number of ſaline ſubſtances of different kinds, ſuffers them to cryſtallize more confuſedly, ſo likewiſe it is found that theſe glaſſes do not preſent the moſt regular cryſtallizations. A precipitation takes place through the whole of the maſs; each of the component parts obeys the laws of affinities; at the ſame time the tranſparency ſpeedily diſappears, and we no longer ſee a piece of glaſs, but a ſtone. Through this confuſed maſs it is nevertheleſs impoſſible not to obſerve the rudiments of cryſtals. Such is the manner in which bottle glaſs comports itſelf in its devitrific action. Theſe approach very much to the ſtate of mere earthy glaſſes, having very little ſalt in their compoſition.

Bottle glaſs.

Experiment in the ſmall way.

Every one has an opportunity of keeping a common bottle of black glaſs in a fire long continued, and capable of ſoftening its paſte; it ſpeedily changes colour, becomes grey, and aſſumes the appearance of ſtone ware. In this manner we form the pottery of Reaumur, but without any proceſs reſembling cryſtallization.

Particular examination of the changes in glaſs by ſlow cooling.

But inſtead of obſerving the phenomenon in ſo ſmall a maſs, if we examine the bottoms of the glaſs furnaces in which theſe bottles are fuſed, we find that the glaſs is abſolutely devitrified, and has aſſumed an appearance ſo completely ſtony, that the moſt experienced eye can ſcarcely diſtinguiſh the bricks of the furnace from the part which has been glaſs. It is only by following the courſe of the devitrification in pieces leſs advanced, that we ſucceed in diſtinguiſhing the glaſs in a granulated ſtone, which has rather the appearance of coarſe pottery, or a ſtrongly baked clay.

Inſtance of the effect taking place in a ſhort time.

It often happens that the ſlow cooling of an hour or two is ſufficient to produce the entire devitrification of bottle glaſs. I have pieces of eight centimetres ($2\frac{1}{2}$ inches) thick, which I obtained at the glaſs works of Mr. Saget, of La Gare. One of the pots was taken out of the furnace to be replaced; the glaſs remaining at the bottom of this pot was preſerved from cooling during the time the pot itſelf acquired the common temperature, and by this means the nature of the glaſs was entirely

entirely changed. It no longer exhibits the tranſparency, but is a maſs of cryſtals, compoſed of ſmall needles, converging towards common centers. It has no longer the appearance of glaſs.

This fact ſhews with what facility bottle glaſs is devitrified, and always without the leaſt appearance of cementation. The infinite variety of ſubſtances which make up the compoſition of bottle glaſs, greatly modifies the phenomena which take place during their devitrification, and muſt, no doubt, influence the form of the cryſtals; but my opportunities of obſerving this kind of glaſs have not been numerous.

Greeniſh glaſs of Alſatia is leſs compounded, and changes more ſlowly. First change, a bluiſh colour.

Paſſing therefore to glaſſes leſs earthy and compoſed of a leſs number of ſubſtances, when I make the ſame examination of the bottoms of the furnaces of glaſs for windows called glaſſes of Alſatia, or half clear glaſſes, into which is put a greater portion of pure ſand and of alkali, I obſerve nearly the ſame phenomenon; but as they are leſs ſudden, they are more eaſy to diſtinguiſh and to ſeparate. At the firſt inſtant, and in the maſſes where the devitrification commences, we ſeem to obſerve a ſmall portion of blue colour diffuſed through a greeniſh liquid.

A very ſingular fact here preſents itſelf, which I ſhall here only point out, with the intention of more fully examining it hereafter. This greeniſh glaſs mixed with blue, appears, in fact, to become of an obſcure blue when conſidered by reflected light; but if placed ſo as to tranſmit the light, it always appears greeniſh; the blue being reflected and the green refracted, each ſingly.

Second change, dull white opacity.

By continuing to obſerve the devitrification of this glaſs, it is ſoon perceived that the blue precipitation is followed by another more abundant, which affords a dirty white, and is very diſtinct from the former. The colour of this glaſs becomes deeper and deeper, and at length it reſembles grey horn.

Third change, needled cryſtals in groupes.

In theſe different tranſitions the paſte of the glaſs appears conſtantly to remain, diſtinguiſhable in its poliſh, its fracture, and all its other properties, except its tranſparency. But in the midſt of this paſte reſembling horn, very diſtinct cryſtallizations are formed, conſiſting of nodules compoſed of ſmall needles, all converging towards the center. In this ſtate it is no longer glaſs, but a cryſtal poſſeſſing all the phyſical properties of mineral ſubſtances left to themſelves.

An

An exact analyſis made of a certain number of theſe cryſtals carefully detached from the maſs, might indicate their nature and throw light on their formation.

It often happens that theſe cryſtallized nodules are enveloped in a cruſt which ſeems foreign to their nature, and may be compared to the cruſt which envelopes flints in the middle of the chalk-banks in which they ſeem to grow.

Speedy refrigeration confuſes the phenomenon.

Theſe are the ſeries of phenomenon attending the devitrification of greeniſh glaſs when it happens ſlowly; but if the effect is too rapidly produced, the appearances reſemble thoſe obſerved in the devitrification of bottle glaſs. The window glaſs here ſpoken of, is of that kind which contains very little of earthy matter, except wood aſhes. It muſt admit of many variations according to the differences in its compoſition.

Clear and ſimple glaſſes are not eaſily devitrified.

White or colourleſs glaſſes are very difficultly cryſtallized or devitrified. When they are well made, we may affirm that a long continued heat does not change them; but for this property it is neceſſary that they ſhould be compoſed merely of ſilex, with the preciſe quantity of flux for its ſaturation. In this caſe, a very long continued fire will no otherwiſe change them than by rendering them more yellow and hard.

(The Concluſion in our next.)

ADVERTISEMENT.

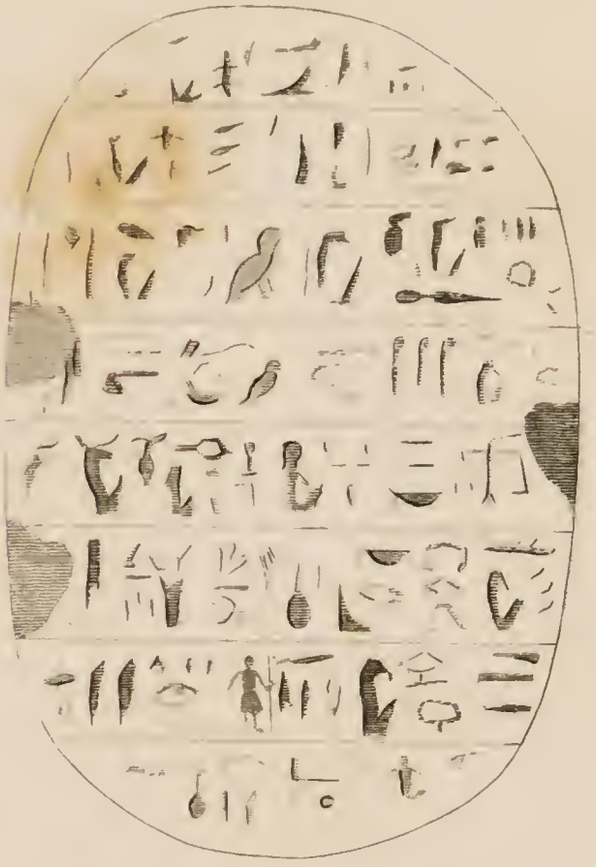
Various articles of Philoſophical News, Accounts of Books, and other ſubjects, have neceſſarily been poſtponed this month, on account of illneſs with which the Editor has been afflicted.

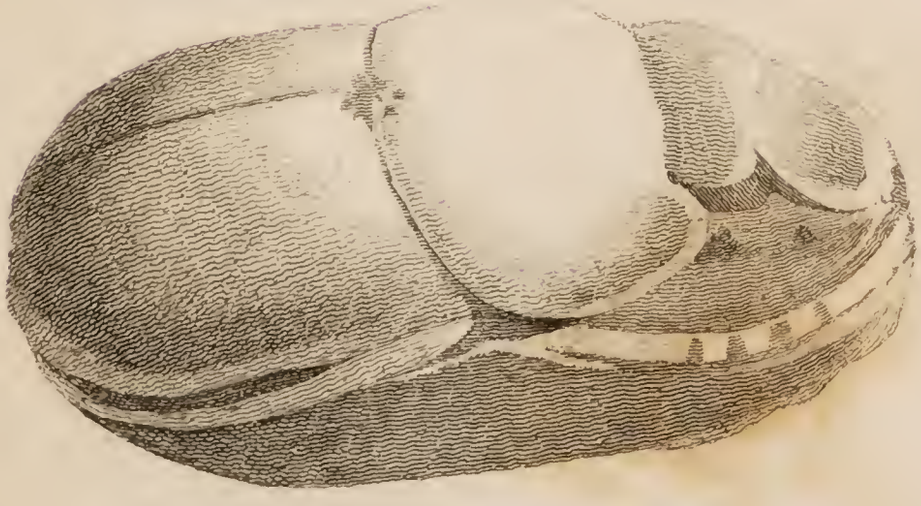
The ſame cauſe has alſo unfortunately prevented his Draughtſman from completing ſome Deſigns, which would elſe have appeared.

Mr. Ezekiel Walker has been prevented, by ſevere indiſpoſition, from anſwering the paper of C. L. in our laſt, upon the Horizontal Moon.

A note has alſo been received from C. L. in which he begs leave to notice, that the large lens mentioned in his experiment I. has a clear aperture of 6,1 *inches in its frame, and was ſo uſed in that experiment.*

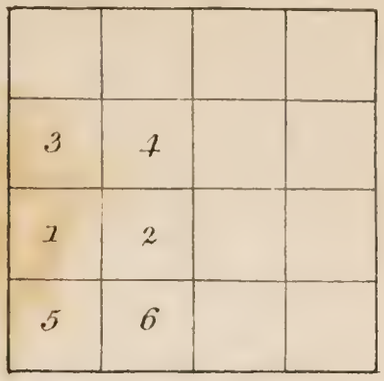
Egyptian Engraving on a Sepulchre probably astronomical.





Galvanic Illustrations.

Fig. 1.



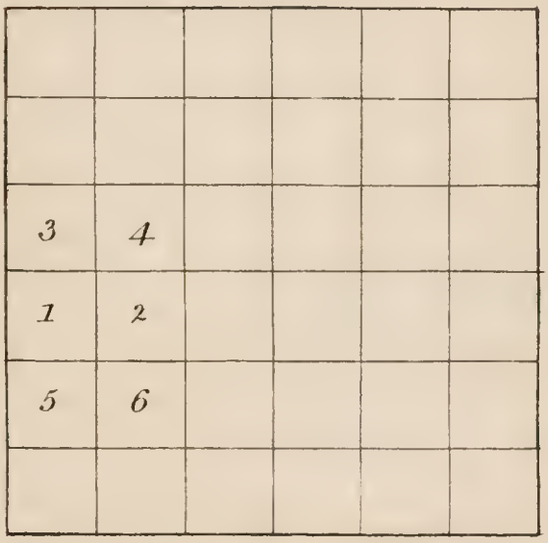


Fig. 2.

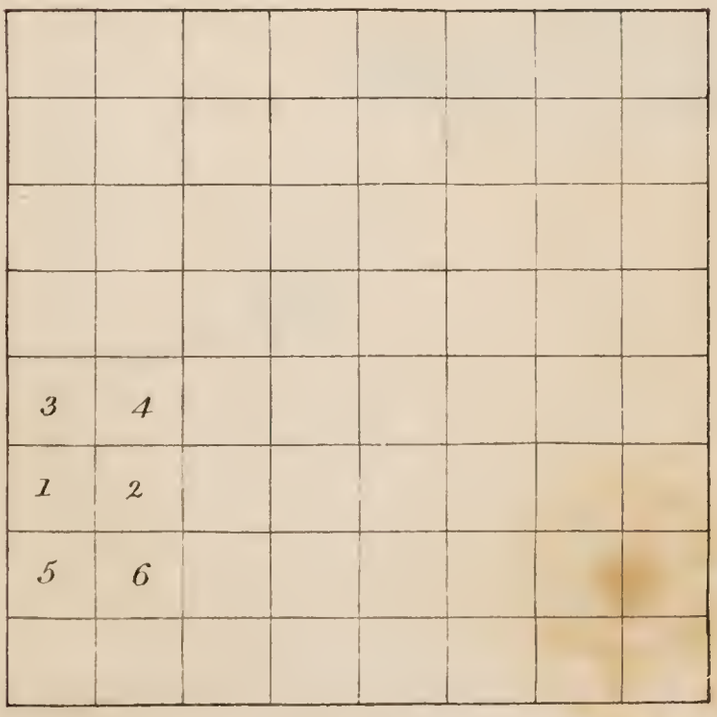


Fig. 3.

Mutlow Sc. Ruffell Ol.

Outlines of Pendulous measures of time
By J. Whitley Boswell Esq.r

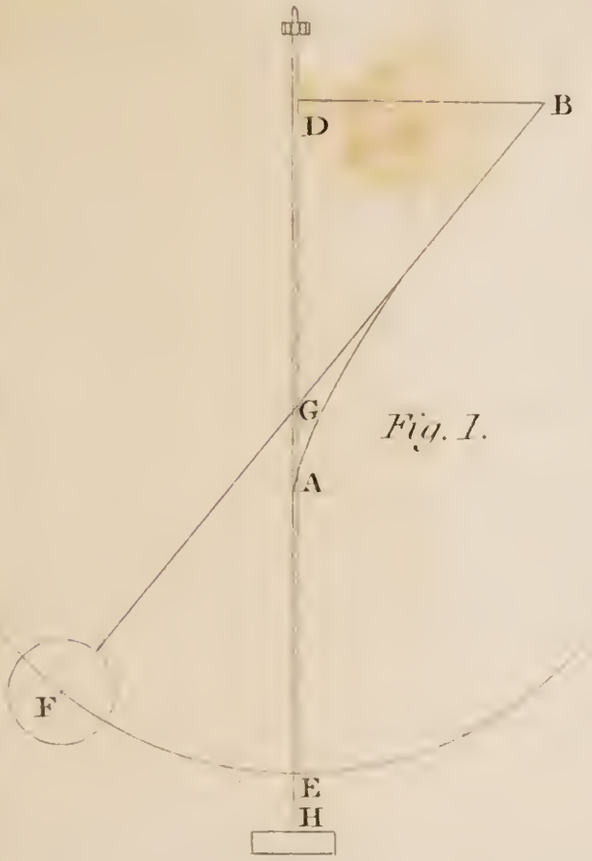


Fig. 1.

Fig. 4.

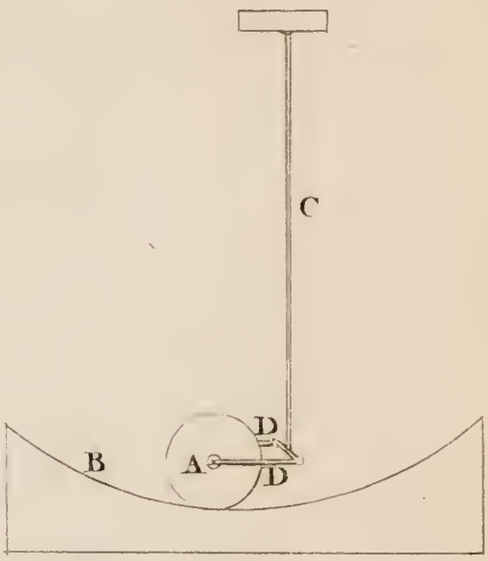


Fig. 2.

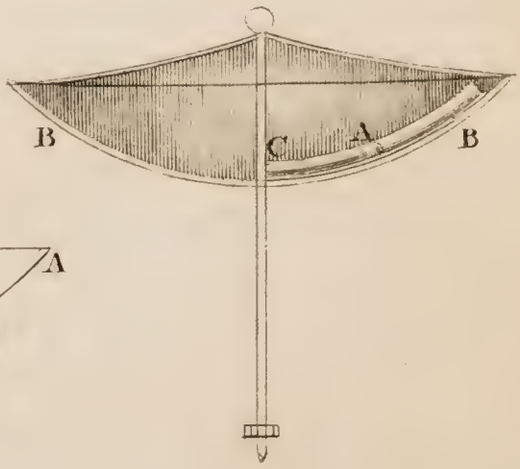


Fig. 3.

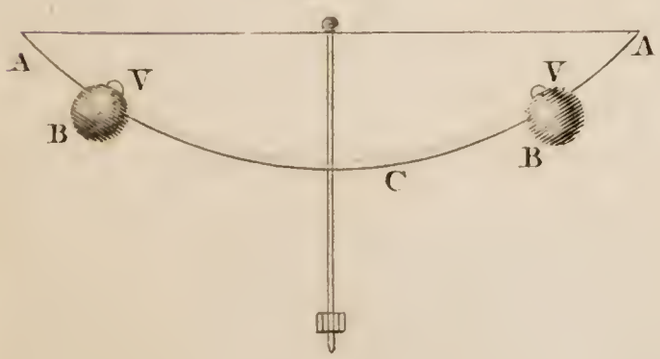


Fig. 5.

Mallow Sc. Rugwell Ch.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

FEBRUARY, 1805.

ARTICLE I.

Experiments and Remarks on the Augmentation of Sounds. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

A FACT is mentioned in a former communication of mine to your Journal (octavo, vol. III. p. 41) which proves, that a ſtroke given to an extenſive vibrating ſurface by a ſlender rod, produces a ſound poſſeſſing a high degree of force: from this it follows, that the range of a ſound may be extended by enlarging the vibrating ſurface, while the magnitude of the impulſe remains the ſame. The preceding maxim is of great uſe in acouſtics; but as the truth of this propoſition reſts at preſent on the authority of a local obſervation, it may not appear ſuperfluous to give a few eaſy experiments in confirmation of the fact.

Sounds heard farther from an extended ſurface.

Experiment I.—My firſt attempt endeavoured to prove, that when a ſound ceaſes to be audible from remoteneſs, the enlargement of the vibrating ſurface will again render it diſtinct at the ſame diſtance. For this purpoſe, a watch was ſuſpended, on a calm day, from the branch of a tree, about $5\frac{1}{2}$ feet from the ground. The clicking of it barely reached my

Exp. 1. The beating of a watch audible at a greater diſtance when in contact with a metallic plate.

ear, in thoſe intervals of ſilence which were not diſturbed by foreign ſounds, at the diſtance of $5\frac{1}{2}$ yards; but when I removed half a yard farther, the ſtrokes of the balance were no longer heard. A circular plate of rolled iron, one foot in diameter, was then brought into contact with the part of the watch fartheſt from me, the poſition of it being ſuch as to preſent the plane of the circle to my ear. This circumſtance increaſed the range of the ſound; for every ſtroke of the inſtrument was diſtinctly perceived at the diſtance of four yards, when the ſilence of the place remained undiſturbed by other cauſes. The reſult of this experiment is too obvious to need a comment: and I have only to add, that when the trial is made within doors, the range of the clicking is greatly increaſed; becauſe the primary ſound receives an acceſſion of force from a number of its own pulſes, which are reflected to the ear by the furniture and walls.

Exp. 2. The plate gives a conſiderable ſound itſelf.

Experiment II.—I proceeded, in the next place, to examine the effects produced on the auditory organs by the vibrations communicated to an elaſtic ſurface by a watch; the immediate ſound of which was ſmothered by art. For this purpoſe, a watch was placed upon a cuſhion, under an inverted porcelain cup, which was wrapped externally in ſeveral folds of flannel. The inſtrument was heard with difficulty at the diſtance of one foot in this ſort of confinement; but when I placed the muffler with the watch under it, upon a ſquare mahogany table four feet broad, the clicking noiſe imparted by it to the top of the table, reached the ear very diſtinctly at the diſtance of four yards. The ſame watch, covered in like manner upon the iron plate mentioned above, reached my ear at the diſtance of 22 feet. The apparatus ſtood in this trial upon a round oak table, 24 inches in diameter.

The enlargement of the ſurface augments the ſound.

The preceding fact proves indiſputably, that the ſecondary vibrations of an elaſtic ſurface actually affect the ear, in thoſe caſes wherein the pulſes never reach the perſon of the hearers, which proceed from the part that is excluſively conſidered as the ſeat of ſound; in other words it proves, that the cauſe of a ſound is not confined to the obſtacle receiving the primary impulſe, but that it is propagated from the point of impact through the contiguous bodies. The firſt experiment alſo ſhews, that the enlargement of the vibrating ſurface encreaſes the force, which a given ſtroke exerts upon an ear placed at

a given diſtance; conſequently the increaſed force thus imparted to a ſound, muſt be aſcribed to the co-operation of the vibrations communicated in the firſt experiment to the plate of iron; for by uniting with thoſe of the watch, they evidently augmented the power of the latter inſtrument.

Theory of the force of ſound augmented by the vibrations of a ſurface touching the ſonorous body.

A rational theory of the forces of ſounds may be formed, by help of the preceding obſervations, in the following manner: *Firſt*, Suppoſe the plane ABC *(Plate IV.)* to be one of the ſurfaces of an elaſtic body, and let it receive an impulſe at the point O; then O will recede from the ſtroke in a right line perpendicular to A B C, drawing after it all the contiguous particles, which, in their turn, will change the place of the next circle of particles. In this manner a circular dent O M E Q T F will be formed; the center of which O will be below the plane A B C; M, the higheſt part of the margin, will be above it; and the extreme circle E Q T F, bounding the whole, will lie in the plane. *Second*, Thus will a circular ſwell be formed on the plane, reſembling the circular wave produced by a ſtone dropped into water. A ſeries of theſe ſwells will follow the firſt, each of which, in ſucceſſion, will lie more remote from the center O than its predeceſſor. *Third*, The collective force of each ſwell is equal to that of the firſt impulſe; and it is diſtributed over the ſurface of a ring, having O for its center and O E for its breadth; *i. e.* all the rings are of equal breadths, but unequal diameter. *Fourth*, The effect of the ſtroke is thus progreſſively propagated from O to the more diſtant parts of the plane A B C, with an uniform velocity. *Fifth*, Let P be the place of the ear in the right line O P, perpendicular to A B C; alſo let G H I J K be any ſwell in the ſame plane; then will the effect of that ſwell be carried to the ear, in the conical ſhell O G I J K P; and the impulſe imparted to the auditory organs, through the medium of this ſhell, will be greateſt when the diameter of the ring O I is leaſt, and the contrary. *Sixth*, If A B C be a concave ſpherical ſurface, having P for its center, all the ſwells will ſucceſſively exert equal forces on the ear at P. *Seventh*, If O P be put equal to a, and f meaſure the force of a ſtroke at the diſtance 1; then let v equal the velocity with which this force is propagated from O over the ſpherical ſurface, and l the breadth of the rings covered by the ſwells, the force impreſſed on the ear at P in a given interval of time, is equal

to $\frac{fv}{la^2}$. *Eighth*, Though time is infinitely divifible in a mathematical fenfe, M. Euler has fhewn *, that the ear conceives it to conſiſt of indiviſible or elementary particles; conſequently all the forces which arrive at P during one of theſe elementary intervals, make a ſingle indiviſible impreſſion on an ear placed at that point; becauſe this organ cannot take cognizance of a ſmaller particle of time; therefore the force of a ſound, produced at the center of a hollow ſphere by the undulations of its ſurface, is truly denoted by the expreſſion $\frac{fv}{la^2}$. *Ninth*, In general, the force of a ſound, at any inſtant of its duration, is equal to the ſum of the forces exerted by thoſe pulſes, which ſtrike the ear in the elementary interval: Conſequently, if the diſtance between the hearer and the ſounding body be ſuch, that lines drawn from his ear to every point of the vibrating ſurface, may be conſidered as equal, the force of the ſound will be expreſſed by $\frac{fv}{la^2}$. *Tenth*, To ſhew the nature of this theorem by an example; ſuppoſe a bell A, the diameter of whoſe mouth $= 1\frac{1}{3}$, to be an octave below a bell B, the diameter of which $= 1$: alſo let A be heard twice as far as B, when each receives an equal ſtroke; the ratio of the breadths of the ſwells in A and B is required. Let V, v, be the velocities of the ſwells in A and B, L, l, the breadths of theſe ſwells; alſo let 2 and 1 be their reſpective ranges: then ſince f is given, $\frac{V}{4L} = \frac{v}{l}$, by the theorem; hence, as $V : 4v :: L : l$. Now the ſemi-circumferences of A and B are in the ratio of $1\frac{1}{3}$ to 1; but while the vibrations paſs over half the circumference of A, they paſs and repaſs over the ſame part of B: therefore, as $V : v :: 1\frac{1}{3} : 2$: hence alſo, as $L : l :: 1 : 6$. Q. E. I.

The principle extended to voices and other ſounds.

Should the principle explained above be admitted, the following concluſion muſt alſo be received as a neceſſary conſequence of it: the voices of animals, as well as the notes of muſical inſtruments, and the reports produced by blows given to leſs elaſtic ſubſtances, derive no inconſiderable portion of their reſpective forces from the vibrations of parts which are

* Tentamen de Mus. Cap. I. Sec. 13.

not

not directly concerned in the production of the primary ſound. This opinion appears to be countenanced by an experiment, which I have repeated at different times under various forms, and of which the following is the ſubſtance: If a wire be ſtretched by two pins fixed into a bad conductor, ſuch as a block of ſtone, the ſound produced by it is much weaker than that of an equal wire ſimilarly ſtretched upon a board, which is a better conductor. In like manner, if a circular piece of wood be ſtruck by a leaden ball conſtantly falling from the ſame height, the report will be heard at a greater diſtance, when the wood is placed upon a good conductor of ſound, than when it reſts upon a bad one. Theſe facts create a high degree of probability, that the leading maxim of this eſſay is applicable to ſounds of every deſcription, embracing ſuch as are continuous, as well as thoſe of a momentary duration.

Similar theory by Dr. Matthew Young.

Dr. Matthew Young's Enquiry into the Phenomena of Sounds has fallen into my hands ſince the foregoing remarks were committed to paper, and a partial peruſal of the work has convinced me, that the preſent theory has not all the claim to originality which I once ſuppoſed to be due to it. The juſtice and neceſſity of the preceding obſervation will appear from the Doctor's two theories of the ſpeaking-trumpet and echoes; in both of which he has made uſe of my leading principle with ſucceſs. The maxim, however, has been extended to a greater number of caſes by theſe obſervations; beſides which, an attempt has been made to demonſtrate the truth of it by experiment, and it is on the two circumſtances here ſtated that the merits of the preſent letter muſt reſt; for I am far from deſiring to rob the celebrated author of the enquiry of the honour due to his ſagacity.

JOHN GOUGH.

Middleſhaw, Jan. 4, 1805.

Obſervations

II.

Observations on the different Degrees of Facility with which Maſſes of the ſame Material admit of Changes in their Temperature; with Applications of the Facts to the Conſtruction of Pendulums, and Speculations upon various new Forms of pendulous Regulators of Time. In a Letter from J. WHITLEY BOSWELL, *Eſq.*

To Mr. NICHOLSON.

SIR,

Compound pendulums.

THE ſatisfactory communication on an ingenious improvement of the compound pendulum, in your Journal for December, has reminded me of ſome ideas which occurred to me on ſimilar ſubjects. In hopes that they may be of ſome uſe in a matter ſo intereſting in itſelf, and ſo important in its application, I ſend them for your opinion.

A very material point ſeems to have been omitted in all the conſiderations I have met with on the expanſion of bodies by heat; pyrometrical experiments being directed to that merely of different ſubſtances of the ſame ſize, but none being made on bodies differing in bulk.

Bodies are more or leſs ſpeedily expand d by heat according to their dimenſions and figure.

Though various bodies differ in their degree of expanſion by heat according to their materials, yet all are ſubject to certain laws on this point, depending on their dimenſions; for as bodies receive or communicate heat by their ſurfaces, and retain it in proportion to their bulk, it follows, that their mutability of temperature muſt depend on the ratio of their ſolid contents to their ſurfaces; and that the greater the ſurface in proportion to the bulk, the more readily will a body change its temperature; and on the contrary, the greater the bulk in proportion to the ſurface, the leſs will it be affected by the fluctuating heat of the ſurrounding medium.

Deduction of the effects.

The proportion which the ſurfaces of bodies bear to their bulk may be varied, either by altering their ſhape or changing their ſize. In the firſt reſpect it is ſufficient to note, that the flatter and longer any body is, the greater will be its ſurface in proportion to its maſs of matter, and *vice verſa*: the difference cauſed by the variation of bulk can be more eaſily calculated, as in bodies of ſimilar figures their ſurfaces are exactly

exactly as the ſquares, and their ſolid contents as the cubes of their diameters. From this, and what has been already premiſed, it will follow, that in bodies of different ſizes, ſimilarly ſhaped, and of the ſame ſubſtance, the capacity for heat will be as the cubes, and the mutability of temperature as the ſquares of their diameters or ſides; and that, therefore, the degree of the tendency of thoſe bodies to maintain an equality of temperature, may be eſtimated as their ſolid contents *minus* the relative value of their ſurfaces: Bodies whoſe ſhapes are diſſimilar, will in ſome degree be ſubject to the ſame rule; but it is not material to the preſent ſubject to take them farther into conſideration.

Table of the rapidity of heating and cooling.

The following table of the proportion of four cubes in the above reſpects, whoſe ſides are reciprocally as 1, 2, 3, and 4, will exemplify what has been above aſſerted, and ſhew how greatly the retention of temperature of bodies is increaſed by adding to their bulk.

	Side.	Surface.	Change of Heat as	Bulk.	Capacity for Heat.	Retention of Heat eſtimated.
	Feet.	Square Feet.		Cubic Ft.		
A	1	6 : 1	1	1	1	1
B	2	24 : 4	4	8	8	4
C	3	54 : 9	9	27	27	18
D	4	96 : 16	16	64	64	48

Inferences.

From theſe conſiderations the following inferences may be drawn: 1*ſt*, That the greater the bulk of any body, the leſs will be its mutability of temperature in proportion, and of courſe the leſs will it alter its degree of expanſion: 2*d*, That a large globe in the firſt place, or a cylinder, whoſe height was equal to its diameter, in the next place, or in the third place a large cube, would have its dimenſions very little changed by the fluctuations of atmoſpherical temperature.

Application of theſe facts to clocks.

To apply theſe principles to the regulation of horological movements may appear difficult, as the firſt idea that would occur is, that it would be neceſſary to put thoſe large bodies in motion for this purpoſe; but this is by no means needful, and it may be effectually performed by conſtructing and fixing up a pendulum in ſuch a manner, that its variations in length ſhall be corrected by, and depend on, thoſe of the large body.

There

Pendulums of Croſthwaite and Pine.

There is no need of much inveſtigation to diſcover the mode of doing this; as either the pendulum invented by Mr. Croſthwaite, of Dublin, or that contrived by Mr. Pine, will fully anſwer this purpoſe, though they will by no means perform that for which they were intended,---of effecting a compenſation of themſelves, in the manner of the gridiron pendulum.

Their miſtake.

The error of theſe gentlemen, in this reſpect, is very fully pointed out in a paper ſigned A. B. publiſhed in the ſeventh Volume of the Repertory of Arts, which would have been much more creditable to its author, had he not triumphed ſo much in his ſuperior penetration on the occaſion, which has led him to forget himſelf ſo far as to deſcend to the illiberality of national reflection on the part of Mr. Croſthwaite: for which reaſon I own I am happy to have it in my power to give theſe gentlemen the ſatisfaction of ſeeing an error pointed out in his paper in return; which is, where he aſſerts that a ſmall rod of mahogany muſt have the ſame contraction and dilation, from change of heat, as a *ſolid plank* of the ſame wood, or words to that effect, the fallacy of which opinion appears from what has been here laid down: It would be eaſy to diſtinguiſh this philoſophy by an appellation of the ſame ſtamp as that which A. B. has beſtowed on Mr. Croſthwaite's invention, and the more unfortunately for his inſinuation on this occaſion, as the Iriſh method happens to be the beſt of the two; for Mr. Croſthwaite ſupported his pendulum by a ſolid wall, which would be much leſs affected by change of heat in the air than the back of the clock-caſe to which Mr. Pine attached his.

Uſe of a large block of ſtone for fixing a pendulum and clock.

The beſt method of applying the foregoing facts to the regulation of clocks appears to me to be, to procure a large cylinder, or octagonal priſm, of ſtone, the diameter of whoſe baſe is equal to its height, and exceeds the length of the pendulum by ſome inches at leaſt, and as much larger as it could be got the better: one of a cubic ſhape would alſo do, if its maſs of matter exceeded that which could be got of the other forms; for this purpoſe granite ſtone ſeems preferable, and in the next place Portland ſtone, as they can be eaſily had in large blocks; but it is probable that ſtones which grow damp in moiſt weather would not be ſo proper for this purpoſe. To this block of ſtone a pendulum ſhould be attached with a ſingle compenſating

compenſating rod added to it, in the ſame manner as that conſtructed by Mr. Croſthwaite or by Mr. Pine: the clock itſelf ſhould alſo be faſtened to the ſtone, particularly if Mr. Croſthwaite's pendulum is uſed.

Defence of the block from change of temperature, &c.

The block of ſtone may be farther ſecured from change of temperature, by being ſurrounded on every ſide by brick-work, except where the pendulum and clock are fixed, and having dry ſaw-duſt rammed in between it and the brick-work. It is probable that a block thus fitted up, would vary little from changes in the heat of the air; and in thoſe ſituations where large blocks of ſtone are found naturally, this might be done at a ſmall expence: where a large block of ſtone could not be procured, a piece of dry ſtraight-grained red deal balk, or ſolid mahogany, might be uſed to good effect, if well painted, and incloſed as before directed: and for common uſe, it is probable that a large glaſs tube, filled with ſemen lycopodii, if to be had, or fine dry ſaw-duſt, well cloſed at either end, and covered with oiled ſilk, would form a pendulum-rod little affected by heat or cold; but if metal is preferred, then a tube of metal, fitted up as laſt directed, would be preferable to the ſmall wires now uſed for this purpoſe.

Advantages.

The advantages to be expected from the principal method above-mentioned, of fitting up pendulums, is, that it affords an unlimited mode of approximation to perfect compenſation; and that, as it requires no great refinement of workmanſhip in its conſtruction, it can be uſed in many ſituations where it would be impoſſible to have a tubular compenſating, or common gridiron pendulum made; which, notwithſtanding the late improvements, are extremely difficult to conſtruct with accuracy, as may be ſeen in the paper publiſhed in your Journal for December laſt, where many of the impediments to their perfection are pointed out, and more ſtill remain to be noticed, of which the following deſerve ſome attention.

Imperfections of compound pendulums ſtated. The metals are mixed, and not conſtant in their compoſition.

The metals of which gridiron pendulums are compoſed, are both of them mixed metals, and of courſe every different parcel of them made, muſt vary in ſome degree in the relative quantities of their component ingredients, and from thence in their degree of expanſion by heat. It may appear ſtrange to call ſteel a mixed metal; but when it is conſidered that charcoal forms a conſiderable part of it, the juſtice of eſtimating it as ſuch will be evident; and in this metal the proportion

portion of charcoal in different parcels of it, is much more various than that of zinc is in braſs: perhaps, for this reaſon, it might be better to uſe ſome of the ſimple metals in the place of theſe, in their conſtruction.

Difficulty of adjuſtment.

Another difficulty in forming theſe pendulums with accuracy, ariſes from the want of a good method of proving them. The adjuſtment by the going of a clock is ſhewn to be imperfect in the paper on the tubular pendulum, and that propoſed in its place, the uſe of the pyrometer, is equally defective, for the following reaſons:

Defects alledged in pyrometers.

Pyrometers are unfit for meaſuring the effects of the heat and cold of the atmoſphere on any ſubſtance, becauſe their own machinery, and particularly the part ſupporting the ſubſtance under trial, is liable to be affected by the ſame cauſes: and in this caſe the index will ſhew the ſum of the alteration of the ſubſtance, the part of the apparatus between its two extremities, and the machinery of the pyrometer, inſtead of that of the ſubſtance alone: and in applying artificial heat to the matter under trial, it is extremely difficult to communicate that heat equally to all its parts at the ſame time, and ſo as not to operate on the pyrometer itſelf, without which the exactneſs of a compound pendulum could not juſtly be tried by it.

Laſtly, another error is produced in the computation of the aberration of all pendulums, from not taking into account the dilation and contraction of the ſmall ſteel ſpring by which they are uſually ſuſpended.

Other pendulums.

As all the oſcillatory pendulums yet made public are influenced in their notation of time by the expanſion of their ſubſtance, and as the beſt contrivances to compenſate this are only an approach to perfection, an increaſe of probability but no certainty, it is therefore an object worthy attention to inveſtigate other methods for effecting the ſame purpoſe: for which reaſon I ſhall here beg leave to ſuggeſt the conſideration of a ſpecies of pendulum of a different nature, which has never been condemned on any juſt ground that I could hear, and which in fact is ſo little known, that the application of a ſimilar movement to other purpoſes, is by many ſuppoſed to be a late invention. The pendulum which I allude to, is that treated of in the fifth Part of the *Horologium Oſcillatorium* of the well known Huygens, publiſhed in 1673, which he calls

calls the circular pendulum, and which much reſembles the centrifugal apparatus, uſed frequently about ſteam-engines to regulate the aperture of the ſteam-damper, of which it probably ſuggeſted the firſt idea. To form the circular pendulum, a ſpindle (H D, *Fig.* 1, *Plate III.*) proceeds perpendicularly from the clock-work, from whence it receives a circular motion round its own axis; to this is affixed a lamina (B G A) of ſome breadth, bent according to a paraboloidal line, by the evolution of which (after it is joined to a certain right line) a parabola is formed, the conſtruction of which is ſhewn in the eighth propoſition of the third part of the above work: This lamina cauſes the ball of the pendulum (F), attached by two threads to its upper extremity (as it circulates), to perform all its revolutions (which will be of greater or leſs extent as the axis moves with greater or leſs force) in the ſurface of a conoidal parabola (F E). Huygens declares, that all the circles performed by the pendulum thus conſtructed, will be iſynchronous, and then ſhews how to proportion the parts of the apparatus, ſo as that each of its revolutions ſhall be performed in a ſecond, or in half a ſecond: He ſays, the only reaſon which cauſed the oſcillatory pendulum to be preferred to this was, that this laſt was more difficult to conſtruct. As this circular pendulum ſeems to poſſeſs the valuable property of correcting the alterations cauſed in it by expanſion, or of rendering them of no conſequence, the difficulty of conſtruction is a matter of no conſequence, eſpecially as there are now to be found artiſts ſo much more excellent in works of this kind than formerly. Circular pendulum of Huygens.

Huygens gives the following character of this pendulum, which I tranſcribe in his own words: "*Plura tamen, hujus quoque generis de quo nunc loquimur, nec ſine ſucceſſu, conſtructa fuere: eſtque in his ſingulare illud, quod continuo atque æquabili motu circumferri cernitur index poſtremus, qui ſecunda ſcrupula deſignat, cum in omnibus aliis horologiis ſubſultim quaſi feratur: Item hoc quoque, quod abſque ſtrepitu ſonoque omni moveantur hoc ratione conſtructa automata.*" He concludes the book with thirteen theorems, *De vi centrifuga ex motu circulari*, ſeveral of which prove this kind of pendulum to have moſt valuable qualities: the ſixth of theſe, being very remarkable, I copy for your readers, who may not have an opportunity of ſeeing the work which contains it, as follows:

"*THEOREMA.*

" *THEOREMA.*

" *In cava ſuperficie conoidis parabolici, quod axem ad perpendiculum erectum habeat, circuitus omnes mobilis, circumferentias horizonti parallelas percurrentis, ſive parvæ ſive magnæ fuerint, æqualibus temporibus peraguntur: quæ tempora ſingula æquantur binis oſcillationibus penduli, cujus longitudo ſit dimidium lateris recti parabolæ genitricis.*"

From the above theorem, and what has been already laid down on the ſubject, it is evident that the circular pendulum is well worthy of a fair trial, and ſhould be recommended to the attention of all ſcientific conſtructors of horological movements.

As this circular pendulum will take up more room than a common one, when this is any conſiderable inconvenience, one of the following conſtructions on the ſame principles (which have occurred to me while writing this communication), may be uſed in its place.

Various conſtructions of the circular pendulum.

The firſt is as follows: To a ſhort ſpindle aſcending from the clock, as before deſcribed, let there be attached a glaſs or iron tube, bent in the form of the conoidal parabola beforementioned, and placed as E F in *Fig.* 1: into this tube let there be poured a ſufficient quantity of mercury to ſerve as a centrifugal weight.

It appears to me that the mercury riſing in the tube, as it circulates, by the centrifugal force, along its parabolical curve, will have the ſame effect as the weight in Huygens's conſtruction, cauſed by other means to move in a ſimilar line. The tube ſhould be ſufficiently large to admit the air to paſs freely above the mercury as it moves along its cavity, or elſe a ſmall aperture ſhould be made in the upper ſurface of the tube near the ſpindle, for the ſame purpoſe: to prevent alſo the error which might otherwiſe be cauſed by a varied reſiſtance to the motion of the circulating tube from the mutability of atmoſpherical preſſure, the tube may be incloſed in a circular veſſel covered at top, made of as light materials as poſſible, and very ſmooth externally, through whoſe axis the ſpindle ſhould paſs, and to which it ſhould be united ſo as to revolve with the tube. A ſecond method of effecting the ſame purpoſe is, to have a ſemi-cylindrical trough ſhaped and placed in ſame manner as the above tube, in which ſhould be put a metallic ſphere of ſmaller diameter than the trough, that the

ſphere

ſphere might move in it without friction; for, being thus formed, it follows, from the 13th of the third book of Euclid, that the ſphere could never touch the trough but in one point alone at the ſame time. A third method of conſtruction is, to place a cylindrical metallic rod, or thick wire, bent into the parabolical curve before directed, in the ſame poſition as the above tube and trough; with a ſpherical weight put on it ſo as to move freely along it, which laſt might be effected by a friction-roller, either let into the upper part of the weight, or placed above it, whoſe ſurface ſhould be formed into a circular groove of larger diameter than the rod.

Thoſe laſt conſtructions ſeem to me to have beſides the farther advantage, that any dilation of their parts from change of temperature, could only lengthen the tubes or rods, but would not alter their ſhape, on which alone their properties depend; which circumſtance might perhaps make them even more exact than that propoſed by Huygens, the ſtrings, or appending part of which, would certainly vary in length; which, though, from the before-recited theorem, it appears to be a matter of little conſequence, yet Huygens ſeems to hint that it would be of ſome, by recommending the uſe of fine chains inſtead of ſtrings (that their length might vary the leſs), in theſe words: "*In locum fili catenulam tenuem exauro, aliove metallo, adhibere licebit, quo melius invariata ſervetur longitudo.*"

There has alſo occurred to me a ſpecies of oſcillatory pendulum; which, as it appears to have the ſame property as thoſe laſt mentioned, (of not being affected by the expanſion of its materials,) I ſhall here deſcribe: It conſiſts of a cylinder of hard metal, A *(Fig. 4)* turned very true and ſmooth, placed ſo as to roll back and forwards alternately, in the cycloidal cavity B, cut in a block of hard metal alſo, and well poliſhed: the communication of motion between the rolling cylinder and the clock work to be effected by the rod C, ſuſpended by its upper extremity, and connected with each extremity of the axis of the cylinder by the joints D D: The diameter of the generating circle of the cycloid being half the length of a pendulum rod, which vibrates in the required time, and the cycloidal part well levelled and firmly ſuſtained. It ſeems to me that the cylinder A, when put in motion, will oſcillate equal times on its cycloidal ſupport; and, (as the expanſion of the latter will not alter its ſhape, on which alone its properties depend,)

An oſcillatory or rolling pendulum.

pend,) that the result will be the same in every different temperature; B may also be formed of two cycloidal plates, having circular grooves hollowed on their edges, and A have two projecting circles turned on it to run in those grooves; the method of connecting A with the clock-work may also be varied many other ways.

Concerning friction, &c.

Before I conclude, I beg leave to observe that the method, before mentioned, of preventing friction in the motion of the sphere in the parabolical trough, might be applied very beneficially in the construction of rail roads, by forming the upper part of the rail of a semi-cylindrical shape, and making in the edges of the wheels, (which were to run on them,) circular grooves, whose diameters should each exceed that of the rail; in the usual way of forming rail-roads, the lateral friction to the wheels is very considerable, whether they have flanches to them, and the rails are made plain, as in the Swansea roads; or the rails are formed with rising ledges, and the wheels plain, as in the Croydon road.

The circular pendulum a good regulator of first movers.

I shall also request permission to mention, in addition to your note on Prony's condenser of forces, that a regulator might be formed on the principles of the circular pendulum, as *Fig. 3*, which would temper the motion of wind-mills, or other engines, and react on them in return, (as well as the fly wheel), in a much more simple and effectual manner than the very complicated apparatus described by Prony for the same purpose, and which seems to me moreover peculiarly liable to be broken by any sudden increase of velocity in the mill, unless formed of most cumberous strength.

I request the favour of having the following typographical errors noted in the paper on my tallow lamp. November, 1804, page 147, line the eighth, for the word *fresh*, read *freely*; and line the 25th, same page, between the words *means* and *horizontal*, insert the letter P. In the plate also, the pan should be deeper and larger than there represented, and the tallow holder more central.

I am, Sir,

Your very humble servant,

J. WHITLEY BOSWELL.

Reference

Reference to the Figures.

Fig. 1. An exact copy of the figure of Huygens's circular pendulum, from the Horologium. B G A, the edge of the paraboloidal lamina: F E, the parabola produced by its means: F, the ball.

Fig. 2. The firſt conſtruction propoſed in place of *Fig.* 1. A C, the parabolical glaſs tube: A, the mercury: C, the air-vent: B B, the encloſing circular veſſel repreſented in ſection.

Fig. 3. The third conſtruction propoſed inſtead of *Fig.* 1. A C, the metallic rod bent into a parabolical figure: B, the ſpherical weight: V, the roller on which it moves along A C: the parts in this figure are repreſented double, to ſhew the beſt method of adopting this plan as a regulator for wind-mills and other engines.

Fig. 4. The propoſed conſtruction for an oſcillatory pendulum, which, it is imagined, will not be affected by change of temperature. A, the oſcillating cylinder: B, the cycloidal ſupport.

Fig. 5. A ſection of the trough mentioned in the ſecond propoſed conſtruction, with the ſphere in it.

Annotations. W. N.

Pyrometers, p. 74.] The common pyrometers of the ſhops are indeed liable to the objections of our author; but ingenious men have long ago obviated them in their experiments. Deluc, in the Philoſophical Tranſactions for 1777, gives a method of aſcertaining the relative expanſions of two different metals by heat. He ſuſpended one of the bars to an arm proceeding horizontally from an upright deal plank, and he ſupported the other bar by reſting its lower end upon a ſmall cock or ſtage proceeding from the lower end of the former. A microſcope was attached to the plank in ſuch a manner that, while it was conſtantly ſupported by an horizontal arm, it could be moved ſo as to keep the laſt mentioned or ſtanding bar in the focus of radiation for diſtinct viſion. Heat was applied to the bars, and the microſcope was directed to a point on the ſtanding bar. When by repeated ſhifting the microſcope a point was found which was neither raiſed nor depreſſed by the changes of temperature, the reſpective lengths of the bars were inverſely as their expanſive powers. There was no mechaniſm, and the plank would remain at the ſame temperature during the experiments,

The beſt pyrometers do not give fallacious reſults.

Deluc's method.

riments, or might be kept ſo by means ſimilar to thoſe pointed out by our ingenious author, or his block of ſtone itſelf might have been uſed.

Ramſden's pyrometer.

That great artiſt Ramſden, whoſe mechanical ſkill and clearneſs of intellect were ſo variouſly diſplayed in his works, and ſtill more in his converſation to thoſe who remember and regret that he has recorded ſo little of the reſults of his labours: this artiſt firſt availed himſelf of microſcopes for aſcertaining the terminal points of the ſubject under examination by his pyrometer; and the interval between one microſcope and the other was rendered permanent by fixing them to arms proceeding at right angles from a bar of iron, kept at 32° by ſurrounding it with melting ice. Phil. Tranſ. Vol. LXXV.

Methods of examining gridiron pendulums.

Laſtly, another error, p. 74.] I believe the ſmall ſpring and ſimple bar part, if any beneath the gridiron, are always taken into the account. Theſe pendulums have not unfrequently been ſubjected to actual examination. An old friend of mine, Mr. J. H. de Magellan had an apparatus of tin, (Anno 1784) in which he incloſed the gridiron when to be examined. It was hung by its ſpring to an arm fixed in an upright plank of deal wood, againſt which plank were attached thermometers to ſhew its ſteadineſs of temperature; and from the center of oſcillation proceeded a (temporary) rod, the end of which preſented a dot as the object to be viewed through a microſcope duly attached to the plank. Without dwelling minutely on this apparatus, I need only obſerve that neither the arm of ſuſpenſion, nor the ſupport of the microſcope, nor the rod from the center of oſcillation had any parts but ſuch as were parallel to the horizon, while the pendulum itſelf was perpendicular to it, and conſequently that no error could ariſe but from expanſion in the plank, which remained at the common temperature; and that the tin apparatus which covered and encloſed the pendulum had only three openings, one at the bottom, one at top, and one through which the rod from the center of oſcillation paſſed, without touching the ſides. In the experiment, ſteam from boiling water was admitted below, and when the heat was ſo raiſed, and the ſupply kept up, that uncondenſed ſteam eſcaped above the pendulum was judged to be at 212°, and if the center of oſcillation continued before the microſcope without either riſing or falling, the compenſation was conſidered as complete.

Magellan's inſtrument.

Mr.

Troughton's pyrometer.

Mr. Troughton's apparatus, alluded to in his paper is different from thoſe, and when he ſhall favour me with the account of it, and the experiments he is making, the world will ſee that it anſwers its purpoſe with ſuch facility and preciſion as do honour to his great ſkill in theſe ſubjects.

Remarks on Huygens's circular pendulum.

Circular Pendulum of Huygens.] The difficulties attending the conſtruction of this pendulum ſeem to be principally in the ſtring or chain. Perhaps fine metallic wire might deſerve the preference. It would not probably be difficult to bring the curve A B ſufficiently near the true figure to anſwer its general purpoſe, through the changes of force in the firſt mover, and of reſiſtance in the air. Practical men have not thought the cycloidal cheeks applied by Huygens, and ſince him by others, to the common pendulum, of any utility; but have rather directed their attention to ſmall vibrations, or an equalizing of the firſt mover by periodically detaching the train. Huygens himſelf alſo propoſed this expedient. It is generally admitted to be an advantage that the regulating inſtrument, whether pendulum or balance, ſhould perform the greateſt part of its motion unconnected with the train. The circular pendulum is conſtantly ſo connected.

Whether it be leſs liable to change from temperature.

As the ingenious author has not detailed his reaſons for thinking that pendulums of this kind will not be affected by change of temperature, I would ſimply remark that it does not appear to me why an expanſion of the ſtring ſhould not cauſe the pendulum of Huygens to revolve more ſlowly, or that an expanſion of the curves in the other figures would not diminiſh their curvature and produce the ſame retardation.

On the method of encloſing the pendulum, &c.

The tube may be encloſed in a circular veſſel.] As the reſiſtance of the air will vary no leſs than a tenth or a fifteenth part according to the ſtation of the barometer, we might expect time pieces to be conſiderably affected by its irregularity. In aſtronomical clocks, with heavy pendulums and ſhort vibrations, this quantity is extremely minute, as is proved by their very correct performance. But there are facts attending the performance of our beſt portable chronometers, which ſhew that it is not in them an inconſiderable object. Several years ago I entertained a notion that the reſiſtance of the air might be rendered equable, or in fact done away, by boxing up the balance, as is here propoſed by our author. But from his own excellent paper on the blaſt ventilator inſerted in the

fourth volume of the quarto ſeries of this Journal, as well as from thoſe of Profeſſor Venturi, inſerted in the ſame work, I learned how greatly the lateral reaction of the air, which is put in motion by bodies paſſing through it, muſt affect their movements. From this cauſe the value of the contrivance will undoubtedly be diminiſhed, but I think not done away. I believe the famous pocket time-piece made about the year 1784, by Emery of Charing-Croſs, for the Preſident Saron at Paris, had its balance boxed up; but as I ſpeak only from recollection, it is quite as probable that the whole movement was encloſed in a cap or caſe ſhut up by a ſecret catch.

III.

Analytical Experiments and Obſervations on Benzoin. By Mr. WILLIAM BRANDE'. *Communicated by the Author.**

Benjamin or benzoes little examined.

THE ſubſtance which forms the ſubject of the following experiments, and which is generally termed Gum-Benjamin or Benzoes, may be enumerated, among thoſe objects, which till lately, have but little engaged the attention of chemiſts.

Mr. Hatchett's experiments.

We are indebted to Mr. Hatchett for almoſt all that is as yet known, as to the chemical properties of the reſins, gum-reſins and balſams; and as the ſubſtance in queſtion, had not been examined by that gentleman, I was induced to make the following attempt.

Benzoin is obtained from the ſtyrax benzoe.

Benzoin is obtained by inciſions made in the tree called *Styrax Benzoe* †, from whence the balſam flows, one tree ſcarcely ever yielding more than three, or at the moſt four pounds.

Two kinds: viz. benzoë amygdaloides, and

Benzoin is generally divided into two kinds; the one is formed of whitiſh yellow tears, reſembling almonds, united

* The following experiments were made merely with a view of aſcertaining the effects of different menſtrua on benzoin; but as the action of ſome of the acids, &c. would have been much leſs intereſting, without a knowledge of the component parts of the balſam, I was induced to make ſome analytical experiments, which, though I fear they are by no means ſo accurate as they ought to be, will, I hope, ſtill be found of ſome uſe.

† Dryander has given a deſcription and drawing of this tree, *vide* Phil. Trans. 1787. p. 307.

together

together by a brown ſubſtance of the ſame nature, and is diſtinguiſhed by the name of benzoë amygdaloides.

The other is a brown ſubſtance, ſomewhat reſembling common reſin, but poſſeſſing in other reſpects, the ſame qualities as the former; it is called common benzoin. common benzoin.

The difference between theſe two ſpecies is ſaid to ariſe from the latter having been expoſed, for a length of time, to the ſun, which converts the white benzoin into the brown. Cauſe of this difference.

The benzoin of the ſhops is uſually in very large brittle maſſes, and ſuch as is whiteſt, and free from extraneous ſubſtances is the moſt eſteemed. It grows in Sumatra, and is brought from the Eaſt Indies only. Benzoin of the ſhops.

When chewed, it impreſſes a ſlight ſweetneſs on the palate; it has however but little taſte. Its ſmell is fragrant and very agreeable, becoming much more perceptible when gently heated. Taſte. Smell.

When thrown on hot coals it firſt fuſes, then takes fire, emitting at the ſame time a ſtrong penetrating odour. Its ſpecific gravity, according to Briſſon, is 1.092. Effects of heat on benzoin. Sp. gr.

Gren enumerates it among the reſins *, as do moſt other authors of his time; it is however more properly denominated a balſam, ſince it is a combination of reſin and benzoic acid. Compoſed of reſin and benzoic acid.

We ſhall firſt examine the effects of different menſtrua on benzoin, and then proceed to its analyſis, together with the methods of obtaining benzoic acid.

§ I.

Effects of different Menſtrua on Benzoin.

1. Cold water has but very little effect on benzoin; boiling water however extracts a part of its acid. Effects of water on benzoin.

2. Alcohol diſſolves the whole of benzoin when digeſted with it in a very gentle heat, the impurities remaining behind. The ſolution is of a deep yellow colour, when perfectly ſaturated, inclining to reddiſh brown, and poſſeſſing in ſome degree the fragrant ſmell of the balſam, which may be obtained pure, either by gentle evaporation of the ſolution or by the addition of water, when a white powder precipitates, formerly known by the name of magiſtery of benzoin. Of alcohol.

The actions of the acids on the ſolution of benzoin in alcohol, are very deſerving of attention. Action of the acids on this ſolution.

* Handbuch, § 1174. 2nd edition. 1794.

Of muriatic acid. A. Muriatic acid being added, a white curd is formed, and when three parts of acid and one of the ſolution are heated together, a very ſmall part of the balſam is rediſſolved, which ſeparates as the mixture cools, while the remainder is converted into a black brittle ſubſtance when cold, part of the benzoic acid having been diſſipated by the heat and part diſſolved, ſome of which ſeparates in the form of a white ſcum, as the mixture cools, together with the benzoin.

Of ſulphuric acid. B. Two or three drops of ſulphuric acid added to the alcoholic ſolution, occaſion a white precipitate, which diſappears on the addition of a ſmall quantity more of acid, the mixture aſſuming the appearance of port wine.

If however equal parts of the ſolution and of ſulphuric acid be mixed together, a dark pink precipitate is formed, the fluid becoming likewiſe of the ſame colour, but the addition of water changes the whole to a lilac colour.

On evaporating a mixture of equal parts of ſolution of benzoin and ſulphuric acid, the liquid retains its beautiful red colour till towards the end of the proceſs, when it becomes gradually darker, and if the evaporation be carried on to dryneſs, a black coaly ſubſtance remains, a decompoſition of the balſam having taken place.

Of nitric acid. C. Nitric acid added to the ſolution of benzoin in alcohol forms a dark red fluid, and when equal quantities are mixed, a violent effervеſcence takes place, attended with the emiſſion of much nitrous gas. It muſt be obſerved that in this inſtance, no precipitate is formed, which proves that the balſam is ſoluble in nitric acid.

Of acetous acid. D. The effects of acetous acid do not exceed thoſe of water.

Of acetic acid. E. Acetic acid forms a precipitate when added to this ſolution of benzoin. The effects produced by acetic acid on benzoin will be noticed hereafter.

Of the alcalies. F. The alcalies form no precipitate in this ſolution of benzoin, although the mixture in ſome caſes becomes ſomewhat turbid.

Action of ether on benzoin. 3. Ether diſſolves benzoin with great facility, the balſam being ſeparated, when this ſolution is agitated with water.

Effects of the acids on this ſolution. The effects of the acids on this ethereal ſolution, ſo nearly coincide with thoſe produced on the ſolution in alcohol, that there will be no need of a detailed account of them.

4. Nitric

4. Nitric acid produces a violent effervescence when poured upon powdered benzoin, the balsam being at the same time converted into an orange coloured mass. Effects of nitric acid on benzoin.

Six ounces of nitrous acid, of the specific gravity of 1.36 being poured on one hundred grains of very pure benzoin reduced into powder, the mixture being gradually heated in a sand bath till it boils, a solution of a light yellow colour is formed. The solution thus obtained, deposits a small quantity of benzoic acid, as it cools: this quantity gradually increases, till after some days, the whole of the benzoic acid appears to have been separated. Solution. Separation of benzoic acid from,

This fact was first observed by Gottling; he does not however seem to have observed that the whole of the balsam is soluble in nitric acid. first observed by Gottling.

A. When the above solution, recently made, is poured into water, the benzoin is precipitated, apparently unaltered. Effect of water on the nitric solution,

B. Sulphuric acid causes no alteration in this solution. of sulphuric acid,

C. Muriatic acid forms a white precipitate which is redissolved on the application of heat, forming a bright yellow liquid, which is a solution of benzoin in nitro-muriatic acid, and in which no precipitate is formed either by water or the alcalies. Its colour is converted into dark brown, when excess of potash is added. of muriatic acid. Sol. of benzoin in nitro-muriatic acid. Effects of water and the alcalies on the nitro-mur. solution.

5. When sulphuric acid is poured on pulverised benzoin, an effervescence takes place; part of the benzoin is dissolved; forming a deep red liquid, and at the same time a coaly substance remains on the surface. Sulphureous acid gas is disengaged during the solution. Effects of sulphuric acid on benzoin.

A. The alcalies form no precipitate in this solution, till after some hours standing, when a dark coloured precipitate is formed. Effects of the alcalies on this solution,

B. Water produces a lilac precipitate in the recent solution. of water.

6. Neither muriatic nor acetous acids have any effect on benzoin. Effects of muriatic and acetous acids on benzoin,

7. Acetic acid dissolves benzoin, even in the cold. The solution formed by the assistance of heat becomes very turbid on cooling, owing to the separation of the benzoic acid. of acetic acid.

A. The alcalies form a white precipitate in this solution.—The precipitate formed in the alcoholic solution, *(page 11)* is re-dissolved after some hours standing. Effects of the alcalies on this solution.

8. Acetic ether dissolves benzoin, without the assistance of heat. Effects of acetic ether on benzoin.

9. When

Effects of ſoda and potaſh on benzoin.

9. When a boiling lixivium of pure potaſh is poured on benzoin, a ſolution is immediately formed; the ſame effect is produced when pure ſoda is made uſe of. Theſe ſolutions are of a dark brown colour, and become turbid after ſome days expoſure to the air.

Effects of the acids on theſe ſolutions.

Precipitates are formed in them by the acids, which are re-diſſolved on the application of heat, when nitric or acetic acids are added in exceſs.

Effects of ammonia on benzoin.

Ammonia likewiſe diſſolves a ſmall quantity of benzoin, from which ſolution it is precipitated by the acids.

Effects of water on the alcaline ſolutions.

No alteration is produced when the alcaline ſolutions are largely diluted with water.*

§ II.

ANALYTICAL EXPERIMENTS ON BENZOIN.

Diſtillation of Benzoin.

Diſtillation of benzoin.

One hundred grains of very pure benzoin were put into a glaſs retort, to which a tubulated receiver, and pneumatic apparatus were adapted; a very gentle heat was firſt applied, which was afterwards gradually increaſed, till the bottom of the retort became red-hot. The products thus obtained were as follows.

Component parts of benzoin.

	Grs.
Benzoic acid	9.
Acidulated water	5.5
Butyraceous and empyreumatic oil	60.
Brittle coal	22.
And a mixture of carbonated hydrogen and carbonic acid gas, which may be computed at	3.5
	100.0

The quantity of benzoic acid above mentioned is that which may be ſeparated by ſublimation only, for on treating the oil, (which amounts to 60 grains) with water, five grains more of acid may be obtained, ſo that 100 grains of benzoin contain

Oil - - - 55
Acid - - 14

* For an account of Mr. Hatchett's experiments on the reſins, gum reſins, and balſams, ſee Thompſon's Syſtem of Chemiſtry, 2d edition, Vol. IV. page 305. for the reſins and balſams to page 328. And from page 341 to 347 for the gum reſins.

The greateſt quantity of benzoic acid is obtained by Chaptals method, which conſiſts in diſtilling all the products over together, and ſeparating the acid by means of boiling water. Chaptal's method of obtaining benzoic acid.

The acid, however, thus obtained, is by no means ſo pure as that procured by Scheele's proceſs *, which is certainly far preferable in many reſpects. Scheele's proceſs.

Another way of obtaining the benzoic acid, is that recommended by Göttling. It conſiſts in digeſting the balſam, in a gentle heat with carbonate of potaſh and precipitation by ſulphuric acid. Gren made uſe of carbonate of ſoda: but it muſt be obſerved that in both theſe proceſſes, the acid is by no means ſo pure as that obtained by Scheele's method, becauſe a far larger proportion of the benzoin itſelf is ſoluble in the fixed alcalies, than in lime water, (the latter only diſſolving a very ſmall portion, to which its brown colour is owing) and Gottling's proceſs for obtaining benzoic acid. Gren's proceſs.

* Scheele's proceſs is as follows: "Upon four parts of unſlacked lime pour 12 parts of water, and after the ebullition is over add 96 parts more of water; then put 12 parts of finely powdered benzoin into a tin pan; pour upon it firſt about ſix parts of the above milk of lime, mix them well together, and thus ſucceſſively add the reſt of the mixture of lime and water. If it be poured in all at once, the benzoin inſtead of mixing with it, will coagulate and run together into a maſs. This mixture ought to be boiled over a gentle fire for half an hour, with conſtant agitation; then take it from the fire, let it ſtand quiet for an hour, in order that it may ſettle; pour off the ſupernatent limpid liquor into a glaſs veſſel. Upon the remainder in the pans pour 96 parts of pure water, boil them together for half an hour, then take it from the fire and let it ſettle; add the ſupernatent liquor to the former; pour upon the reſiduum ſome more water, boil it as aforeſaid, and repeat the ſame proceſs once more. At laſt put all the reſiduums upon a filter, and pour hot water ſeveral times upon it. During this proceſs, the calcareous earth combines with the acid of benzoin, and ſeparates it from the reſinous particles of this ſubſtance. A ſmall quantity of the reſin is diſſolved by the lime water, whence it acquires a yellow colour. All theſe clear leys and decoctions are to be mixed together and boiled down to 24 parts, which are then to be ſtrained into another glaſs veſſel. After they are grown cold, muriatic acid is to be added, with conſtant ſtirring, till there be no further precipitation, or till the maſs taſtes a little ſouriſh. The benzoic acid which was before held in ſolution by the lime, precipitates in the form of a fine powder." Vide Thomſon's Chemiſtry, Vol. II. page 123.

which

which is precipitated by the diluted ſulphuric acid. The following table ſhews the quantity of benzoic acid, obtained by the different proceſſes, from one pound of benzoin.

	Oz.	Dr.	Scr.	Grs.
Scheele's proceſs - - - - -	1	6	2	19
Chaptal's - - - - - -	2	0	0	0
By boiling benzoin with water - -	1	0	0	10
By Gren's and Gottling's proceſſes -	1	5	1	10*

The acid of benzoin, firſt deſcribed by Blaiſe de Vigenere.

Benzoic acid is deſcribed by Blaiſe de Vigenere, as long ago as the year 1608, in his treatiſe on fire and ſalt; he called it flowers of benzoin, becauſe it was obtained by ſublimation. We are indebted to Tromſdorf and Lichtenſtein for many facts relating to this acid.

Properties of the Acid of Benzoin.

Taſte of the acid. Smell.

This acid when obtained according to Scheele's proceſs, is a light yellowiſh powder. Its taſte is hot and rather bitter. It poſſeſſes a ſomewhat fragrant ſmell, ariſing from a ſmall portion of the aromatic oil, which ſtill adheres to it, it has however been obtained without any ſmell by Gieſe.

Properties in general.

It reddens tincture of turnſole. It is volatilized by a gentle heat, being converted into a white ſmoke, which excites a very diſagreeable ſenſation in the throat. When melted, it becomes as fluid as water, and aſſumes a radiated ſurface on cooling. Its ſpecific gravity is 0.667.

Decompoſition.

When diſtilled in cloſe veſſels, a part of it is decompoſed, which is converted into oil and carbureted hidrogen gas.—

Effects of oxigen gas, &c. on this acid.

Tromſdorf† found that it was not altered by oxigen gas, nor by the ſimple combuſtibles or incombuſtibles.

Of ſulphuric acid.

It is ſoluble in ſulphuric acid, which it converts into a brown liquid, and from which it is ſeparated by the addition of water.

Of nitric and ſulphurous and nitrous acids.

The ſame effects are produced by nitric acid, and by ſulphureous and nitrous acids.

Effects of muriatic acid, &c.

It is not acted upon by muriatic, oxymuriatic, or phoſphoric acids. It is eaſily ſoluble in alcohol, from which it is precipitated on the addition of water. Its

* Of pure acid, obtained by treating the precipitate with boiling water.

† See Tromſdorf's experiments on the benzoic acid, in his Journal der Pharmacie.

affinities

affinities appear to be as follows:*—White oxide of arſenic, potaſs, ſoda, ammonia, barytes, lime, magneſia, alumina. Its affinities.

Properties of the Oil of Benzoin.

The oil obtained by the diſtillation of benzoin poſſeſſes a ſtrong empyreumatic odour; but when rectified by a ſecond diſtillation, its ſmell is exceedingly fragrant and pleaſant. Its taſte is acrimonious and very diſagreeable. Smell and taſte,

When diſtilled with water, it imparts ſome of its taſte to that fluid. It is perfectly ſoluble in alcohol, the ſolution becoming turbid when water is added. imparted to water. Solution in alcohol.

Sulphuric acid decompoſes it in part, when the mixture is heated. Nitric acid acts on it as on the eſſential oils in general, converting it into a reſinous ſubſtance. It is not affected by muriatic acid, but is partially ſoluble in acetic acid. It ſinks in water; and forms ſaponaceous compounds with the alcalies. Action of ſulphuric acid. Of nitric acid. Of muriatic acid, and the alcalies.

Such are the general properties of the acid, and oil of benzoin, much more might undoubtedly be ſaid concerning them; but as the foregoing experiments, were not made with a view of inveſtigating their properties, it would be unneceſſary to give a long account of them in this place. It muſt be obſerved that the pureſt benzoin has been made uſe of throughout; and laſtly, that no traces of alcali, were found in the coal which remains after diſtillation.

Arlington-Street.

IV.

On the Devitrification of Glaſs and the Phenomena which happen during its Cryſtallization. *By* DARTIGUES.

(Concluded from Page 64.)

WHEN clear glaſſes contain a certain quantity of a neutral ſalt which the fire has not had time or force to diſſipate, it often happens, during ſlow cooling, that the appearances called greaſe, threads, bubbles, and ſpecks, are ſpontaneouſly and ſuddenly formed. Imperfect glaſſes.

* This table of affinities was formed according to the experiments of Tromſdorff. Vide Thomſon's Syſtems of Chemiſtry, Vol. II. page 126. 2nd edition.

Theſe

Theſe accidents, their different cauſes and remedies, are treated at length in the firſt part of my work, where I ſpeak of the fonding of glaſs; but though the explanation of theſe facts entirely belongs to the theory of devitrification, it will be ſufficient in this place to mention the phenomena which ariſe from the preſence of the different earths.

Cryſtallization of lime in glaſs,

Colourleſs glaſſes contain more or leſs of lime, for the reaſons which I have mentioned in ſpeaking of the different compoſitions of glaſs. This lime, when in exceſs, gives cryſtals very well, as Loyſel remarks: they are eaſily known, and ſo abundant, that they abſolutely impair the tranſparency: they are priſms, which ſeem to float in the midſt of the paſte of glaſs, and tend to unite in ſtars differently figured: theſe priſms are all nearly of the ſame thickneſs, and two or three millimetres in length (about one-tenth of an inch).

in large maſſes.

When this cryſtallization takes place ſpontaneouſly upon large maſſes at the bottom of the furnace, the colour of the glaſs becomes darker, and inclines to black, by the mixture of a certain quantity of the aſhes. The ſtriated ſtars here ſpoken of, become more numerous, the more remote they are from the ſide in contact with the fire. Cryſtallizations at firſt inſulated, are ſoon ſucceeded by a maſs entirely cryſtallized, in which the vitreous character is no longer diſtinguiſhable.

Modified cryſtallizations in glaſs.

Theſe are the moſt common characters of cryſtallization; but others are frequently ſeen, which are certainly owing to accident, and well deſerve notice on account of their varieties*. I have ſome pieces of glaſs in which cryſtals are diſcernable, ſo fine, that they can ſcarcely be ſeen with a magnifier. They are priſms divergent from the ſame center, and forming ſtars, which frequently do not exceed one millimetre (or half a tenth of an inch) in diameter: their union reſembles a ſlight miſt in the paſte of the glaſs.

Some pieces have the aſpect of a ſaline cruſt applied to a foreign body, againſt which the glaſs was in contact. In ſome of theſe this cruſt being compoſed of ſtriated protuberances, appears to advance more and more into the glaſs itſelf.

Laſtly, there is another variety perhaps more curious than all the reſt, in which centers, or cryſtallizations reſembling

* Mr. Sage poſſeſſes a piece of glaſs cryſtallized in ſix-ſided baſaltic priſms, totally devitrified.

peas,

peas, and almoſt ſimilar to grain, are ſeen in the midſt of the paſte of glaſs. They are ſmall ſpheres flattened at each end with an indentation in the middle of each depreſſion. The ſides have facetts, like the grain of Indian naſturtium, and theſe faſſetts are always ſix in number.

I intend, when I ſhall have procured a ſufficient number of this ſingular ſpecies of cryſtal, to analyze it, in order to determine which of the earths it is that effects ſo extraordinary a form.

I have thus given a ſhort account of the principal facts which characterize the precipitation and cryſtallization of glaſs. It is evident that they are of the ſame nature as thoſe produced by the cementation pointed out by Reaumur; and that this cementation is always a more or leſs abſolute devitrification of the glaſs.

Recapitulation.

When the glaſs is devitrified, it has no longer a vitreous but a granulated fracture; it has no tranſparency, and perfectly reſembles a ſtone; it becomes a leſs bad conductor of heat and of electricity; laſtly, it is no longer ſuſceptible of fuſion at the ſame degree of heat; and in order to reſtore it more eaſily to the vitreous ſtate, it muſt firſt be pulverized, to bring into contact thoſe ſubſtances which, during the cryſtallization, had become ſeparated from each other, and could no longer ſerve as fluxes to each other.

Revitrification.

General concluſion and application to geology.

I hope the novelty of many of the facts I have here indicated, and the conſequences I have drawn from them, may be ſufficiently intereſting to philoſophers to have ſome claim to their attention. I have no doubt but that other general inductions will preſent themſelves to them which may have eſcaped me, or which could not be introduced in a ſhort memoir. The reſemblance of my ſpecimens of devitrified glaſs with thoſe of certain lavas; the poſſibility that other lavas may have undergone a more complete devitrification by a much ſlower cooling beneath the volcanic currents which are known to have flowed, or to have remained fluid for whole years beneath congealed cruſts;—every fact leads me to think, that theſe phenomena may afford a ſolution of many geological events, concerning which no general opinion has yet been adopted, becauſe there was no reaſon to believe that ſtones may have formerly poſſeſſed the vitreous ſtate.

Letter

V.

Letter from a Correſpondent, relating to the apparent Reflection of Cold and the Invention of the Teleſcope, as noticed by Sir H. C. ENGLEFIELD *in laſt Month's Journal.*

To Mr. NICHOLSON.

SIR,

IN juſtice to Profeſſor Pictet, I truſt you will have the goodneſs to inſert in your next number the words of a note in his *Eſſai de Phyſique*, p. 81, where he deſcribes his experiment on the apparent reflection of cold:

Apparent reflection of cold.

"*Les Académiciens* del Cimento *avoient eſſayé de concentrer le froid au foyer d'un miroir concave; mais ils avouent eux-mêmes, que leur expérience avoit été faite d'un manière trop inexacte pour qu'on pût en rien conclure de certain.*"

Perhaps Profeſſor Pictet ſpeaks too lightly of the experiment of the Academicians; but ſo far was that experiment from being forgotten, that the account of it has been very lately reprinted in the firſt volume of the Journals of the Royal Inſtitution, p. 224; a work to which your correſpondent, who has revived the ſubject (page 1 of your laſt Number), has himſelf contributed ſome valuable papers.

Kepler did not invent or know of the teleſcope before Galileo.

With reſpect to the invention of the teleſcope, give me leave to obſerve, that *perſpicillum* means ſimply the ſight of an aſtronomical inſtrument, having a ſmall perforation. If the comet in queſtion was ſurrounded by a ſmall round and well-defined nebuloſity, ſuch a ſight might eaſily produce the effect attributed to it. A teleſcopic ſight is uſually called *perſpicillum vitreum*. Galileo ſays, in a work publiſhed in 1610, that he was informed of the invention of teleſcopes about nine months before. Kepler wrote, in 1611, a valuable work on the theory of dioptrics, in which the conſtruction of the aſtronomical teleſcope is deſcribed. Had he been the original inventor of the Galilean teleſcope, he would not omitted this opportunity of ſubſtantiating his claim to ſo important a diſcovery.

I am, Sir,

Your very obedient ſervant,

ALETES.

Facts

VI.

Facts tending to decide the Queſtion, at what Point of Temperature Water poſſeſſes the greateſt Denſity. In a Letter from Mr. JOHN DALTON.

To Mr. NICHOLSON.

SIR,

ACCORDING to the doctrine in moſt of our books treating of the elements of chemical ſcience, it is conſidered as a decided fact, that water is of the greateſt denſity at or near 40° of temperature of Fahrenheit's ſcale; and that, above or below that temperature, it expands alike by heat or cold for a certain number of degrees. I have taught a different doctrine for ſome time paſt; which is, that water is denſeſt at 32°, or the freezing point, and that it expands alike above and below that point for at leaſt 25° (provided it does not freeze), and that the quantity of expanſion is as the ſquare of the temperature, reckoned from 32°, up or down; that is, if the expanſion from 32 to 42° be denoted by 1, that from 32 to 52° will be denoted by 4, and that from 32 to 62° by 9, *&c.* or nearly ſo; the deviation from ſtrict accuracy being, as I conceive, occaſioned by the mercurial thermometer not being an exact meaſure of temperature: the expanſion at 22° is likewiſe 1, at 12° it is 4, and at 2° it is 9, *&c.*; the ſame below as above the freezing point. In conſequence of this, a gentleman of profeſſional eminence has undertaken to examine the ſubject anew; and has been led, by an ingenious train of experiments totally different from the following, to decide in favour of the common notion, that water is denſeſt about 40°. Theſe experiments of his will ſoon be publiſhed. I am ſtill, however, decidedly convinced, that my opinion above ſtated is true, and that principally from the facts I am about to ſtate, which thoſe who adopt the common opinion muſt ſee the abſolute neceſſity of explaining upon their principle, or otherwiſe, of controverting the facts themſelves. As this ſeaſon of the year is moſt convenient for experiments of this nature, I am the more ſolicitous to have the facts announced, leaving all explanation for the preſent. They are of a very ſimple nature, and may be inveſtigated with little trouble and expence.

General notion that water is denſeſt at 40°, &c.

The author's doctrine that 32° is the point of greateſt denſity.

Experiments brought to ſupport the former concluſion.

The author retains his opinion, and invites other experiments.

A number

Inſtructions for experiments. Procure large bulbed thermometers, of earthen ware, glaſs, metal, &c.

A number of water thermometers are to be procured, the containing veſſels of which are of different materials, as earthen ware, glaſs, and various metals. Each of theſe ſhould contain one or two ounces (from 4 to 800 grains), more or leſs, of water. Common brown inkſtands, which go by the name of Nottingham ware, anſwer very well for one ſpecies, but they require to be well painted without, as they are not otherwiſe water-tight. I have a few of Queen's ware, made purpoſely in Staffordſhire, which conſtitute another ſpecies of earthen ware; ſome of them are glazed in and out; others unglazed, but theſe being painted without are made water-tight, and expand the ſame by heat as glazed ones: Of glaſs, common thermometer tubes, with larger bulbs than ordinary, are ſufficient. I have the metallic veſſels made in the ſhape of cylindrical tin canniſters, conical towards the top, and at the ſummit a ſmall cylindrical tube, ſuch as to take a thermometer tube. The glazed earthen ware and the metallic, require moſtly to be painted before they are quite tight.

Fill them with boiled water.

The veſſels being thus prepared, they are to be filled with water previouſly boiled to expel the air; a thermometer tube with cement is then ſuddenly plunged into the veſſel and cemented faſt; the water may then be driven out of the tube by heat, or more may be put into it by a ſmall wire; it is then fit for uſe, and a ſcale of equal parts may be applied to the tube; or it may be divided, and marks made with a file or paint.

Some of the reſults of my experiments with inſtruments of this kind, are as follow:

Experimental reſults noted with three earthen, one glaſs, two iron, one copper, one braſs, one pewter, and one leaden thermometer.

	Water loweſt.	Water the ſame height.
1. Brown earthen ware, No. 1,	at 36°	at 32° and 40°
2. Brown earthen ware, No. 2,	38	32 and 44
3. Queen's ware,	40	32 and 48
4. Flint-glaſs,	41½	32 and 51
5. Iron, thin plate,	42½	32 and 53
6. Tinned iron,	42½	32 and 53
7. Copper,	45½	32 and 59
8. Braſs,	46	32 and 60
9. Pewter,	46	32 and 60
10. Lead,	49½	32 and 67

Another

Another phenomenon in these instruments is observable; it is not new, but it deserves a marked attention in the present enquiry: If the apparent expansion of water for the first 10° of temperature, reckoned from the lowest point in any of the above instruments, be denoted by 1; then if the instrument, taken at any temperature, be suddenly plunged in water of 10° higher temperature, the water instantly *sinks* a considerable way, occasioned no doubt by the vessel being extended by the heat before the water it contains has time to expand. The quantity of depression in the different instruments was found as under:

Phenomenon of the depression of a thermometrical fluid when the instrument is suddenly heated.

Table of this depression in the different thermometers.

Brown earthen ware sinks by being dipped in water of 10° higher temperature, -	.2
Queen's ware, - - - -	.3
Flint-glass, - - - - -	.25
Iron, - - - - - -	.66
Copper, - - - - -	.9
Brass, - - - - -	1.1
Pewter, - - - - -	1.0
Lead, - - - - -	1.5

I submit these facts to the consideration of those who feel interested in the enquiry; and desire that they may be particularly investigated by those who maintain that water is densest at 40°.

I remain your's,

J. DALTON.

Manchester, Jan. 10, 1804.

VII.

Analytical Experiments and Observations on Lac. By CHARLES HATCHETT, *Esq. F. R. S. From the Philosophical Transactions for* 1804.

(Concluded from p. 54.)

Properties of the colouring Extract of Lac.

Colouring extract of lac.

1. WHEN dry, it is of a deep red colour, approaching to purplish crimson.

2. Being put on a red-hot iron, it emits much smoke, with a smell somewhat resembling burned animal matter, and leaves a very bulky and porous coal.

3. Water,

Colouring extract of lac.

3. Water, when digeſted with it in a boiling heat, partially diſſolves it; but the reſiduum was found to be abſolutely inſoluble in water.

4. Alcohol acts but ſlowly on it; and, in a digeſting heat, diſſolves leſs than water. The colour of the ſolution is alſo not ſo beautiful; and a conſiderable part of the reſiduum left by alcohol was, when digeſted with water, found to be ſoluble, although this was not the caſe, when the reſiduum left by water was treated with alcohol.

5. It is inſoluble in ſulphuric ether, excepting a very ſmall portion of reſin, which appeared to be accidentally mixed with it.

6. Sulphuric acid readily diſſolves it, and forms a deep browniſh-red ſolution, which, being diluted with water, and ſaturated with potaſh, ſoda, or ammonia, becomes changed to a deep reddiſh-purple.

7. Muriatic acid diſſolves only a part: the ſolution is of the colour of port-wine, and, by the alkalis, is changed to a deep reddiſh-purple.

8. Nitric acid ſpeedily diſſolves it: the ſolution is yellow, and rather turbid; but the red colour is not reſtored by the alkalis, for theſe only deepen the yellow colour. This nitric ſolution did not afford any trace of oxalic acid.

9. Acetic acid diſſolves it with great eaſe, and forms a deep browniſh-red ſolution.

10. Acetous acid does not diſſolve it quite ſo readily, but the ſolution is of a brighter red. Both of the above, when ſaturated with alkalis, are changed to a deep reddiſh-purple.

11. The lixivia of potaſh, ſoda, and ammonia, act powerfully on this ſubſtance, and almoſt immediately form perfect ſolutions, of a beautiful deep purple colour.

12. Pure alumina, put into the aqueous ſolution, does not immediately produce any effect; but, upon the addition of a few drops of muriatic acid, the colouring matter ſpeedily combines with the alumina, and a beautiful lake is formed.

13. Muriate of tin produces a fine crimſon precipitate, when added to the aqueous ſolution.

14. A ſimilar coloured precipitate is alſo formed, by the addition of ſolution of iſinglaſs.

Theſe properties of the colouring ſubſtance of lac, eſpecially its partial ſolubility in water and in alcohol, and its inſolubility in ether, together with the precipitates formed by alumina and muriate of tin, indicate that this ſubſtance is vegetable extract, perhaps ſlightly animalized by the coccus.

The effects which it produced on gelatin, alſo demonſtrate the preſence of tannin; but this very probably was afforded by the ſmall portions of vegetable bodies, from which the ſtick lac can ſeldom be completely ſeparated.

Properties of the Reſin of Lac.

This ſubſtance is of a browniſh-yellow colour; and, when Reſin of lac. put on a red-hot iron, it emits much ſmoke, with a peculiar ſweet odour, and leaves a ſpongy coal.

It is completely ſoluble in alcohol, ether, acetic acid, nitric acid, and the lixivia of potaſh and ſoda.

Water precipitates it from alcohol, ether, acetic acid, and partially from nitric acid; and it poſſeſſes the other general characters of a true reſin.

Properties of the Gluten of Lac.

It has been already obſerved, that when ſmall pieces of ſhell Gluten of lac. lac have been repeatedly digeſted in cold alcohol, they become white, bulky, and elaſtic. By drying, theſe pieces become browniſh and brittle; the elaſticity is alſo deſtroyed by boiling water, exactly as when the gluten of wheat is thus treated.

If the pieces of ſhell lac, after the digeſtion in alcohol, be digeſted with diluted muriatic acid, or with acetic acid, the greater part of the gluten is diſſolved, and may be precipitated, in a white flaky ſtate, by alkalis; but, if theſe laſt be added to exceſs, and heat be applied, then the glutinous ſubſtance is rediſſolved, and may be precipitated by acids.

If the pieces of ſhell lac, after digeſtion in alcohol, be treated with alkaline lixivia, then the whole is diſſolved, and forms a turbid ſolution. But, when acids are employed, the chief part of the gluten is alone acted upon, and a conſiderable reſiduum is left, conſiſting of the wax, ſome

of the reſin, and a portion of gluten, which has been protected from the action of the acid by the two former ſubſtances.

The above properties indicate a great reſemblance between this ſubſtance and the gluten of wheat; I therefore have called it gluten, but, at a future time, I intend to ſubject it to a more accurate examination.

Properties of the Wax of Lac.

Wax of lac. If ſhell lac be long, and repeatedly digeſted in boiling nitric acid, the whole is diſſolved, excepting the wax, which floats on the ſurface of the liquor, like oil, and, when cold, may be collected; or it may be more eaſily obtained in a pure ſtate, by digeſting the reſiduum left by alcohol in boiling nitric acid.

The wax thus obtained, when pure, is pale yellowiſh white, and (unlike bees wax) is devoid of tenacity, and is extremely brittle.

It melts at a much lower temperature than that of boiling water, burns with a bright flame, and emits an odour ſomewhat reſembling that of ſpermaceti.

Water does not act upon it, neither does cold alcohol; but this laſt, when boiled, partially diſſolves it, and, upon cooling, depoſits the greater part; a ſmall portion, however, remains in ſolution, and may be precipitated by water.

Sulphuric ether, when heated, alſo diſſolves it; but, upon cooling, nearly the whole is depoſited.

Lixivium of potaſh, when boiled with the wax, forms a milky ſolution; but the chief part of the wax floats on the ſurface, in the ſtate of white flocculi, and appears to be converted into a ſoap of difficult ſolubility; it is no longer inflammable, and, with water, forms a turbid ſolution, from which, as well as from the ſolution in potaſh, the wax may be precipitated by acids.

Ammonia, when heated, alſo diſſolves a ſmall portion of the wax, and forms a ſolution very ſimilar to the former.

Nitric and muriatic acids do not ſeem to act upon the wax; the effects of ſulphuric acid have not been examined.

When the properties of this ſubſtance are compared with thoſe of bees-wax, a difference will be perceived; and, on the contrary,

contrary, the moſt ſtriking analogy is evident, between the wax of lac and the myrtle wax which is obtained from the Myrica cerifera. It greatly reſembles myrtle wax.

An account of the latter ſubſtance has been publiſhed by Dr. Boſtock, of Liverpool, in Nicholſon's Journal, with comparative Experiments and Obſervations on bees-wax, ſpermaceti, adipocire, and the cryſtalline matter of biliary calculi. *

The properties of the myrtle wax, as deſcribed in Dr. Boſtock's valuable paper, ſo perfectly coincide with thoſe which I have obſerved in the wax of lac, that I cannot but conſider them as almoſt the ſame ſubſtance; indeed I think they may be regarded as abſolutely identical, if ſome allowance be made for the ſlight modifications which have been produced by the different mode of their formation.

From the preceding experiments and analyſes we find, that the varieties of lac conſiſt of the four ſubſtances which have been deſcribed, namely, extractive colouring matter, reſin, gluten, and a peculiar kind of wax. Reſin is the predominant ſubſtance; but this, as well as the other ingredients, is liable, in a certain degree, to variation in reſpect to quantity. Component parts of lac.

According to the analyſes which have been deſcribed, one hundred parts of each variety of lac yielded as follows:

Stick Lac.

Reſin - - - -	68.
Colouring extract - - -	10.
Wax - - - -	6.
Gluten - - - -	5.50
Extraneous ſubſtances - -	6.50
	9.60.

Seed Lac.

Reſin - - - -	88.50
Colouring extract - - -	2.50
Wax - - - -	4.50
Gluten - - - -	2.
	97.50.

* Nicholſon's Journal for March, 1803, p. 129.

Shell Lac.

Reſin - - - -	90.90
Colouring extract - - -	0.50
Wax - - - -	4.
Gluten - - - -	2.80
	98.20.

The proportions of the ſubſtances which compoſe the varieties of lac, muſt however be ſubject to very conſiderable variations; and we ought therefore only to conſider theſe analyſes in a general point of view. Hence we ſhould ſtate, that lac conſiſts principally of reſin, mixed with certain proportions of a peculiar kind of wax, of gluten, and of colouring extract.

The relative quantity of the two latter ingredients, very conſiderably effect the characters of the lacs; for inſtance, we may obſerve, that the glutinous ſubſtance, when preſent in ſhell lac in a more than uſual proportion, probably produces the defect obſerved in ſome kinds of ſealing wax, which, when heated and burned, become blackened by particles of coal; for the gluten affords much of this ſubſtance, and does not melt, like the reſin and wax. From what has been ſtated, therefore, lac may be denominated a *cero-reſin*, mixed with gluten and colouring extract.

§ III.

GENERAL REMARKS.

Lac has the characters of vegetables.

From the whole of the experiments which have been related, it appears that although lac is indiſputably the production of inſects, yet it poſſeſſes few of the characters of animal ſubſtances: and that the greater part of its aggregate properties, as well as of its component ingredients, are ſuch as more immediately appertain to vegetable bodies.

It is very uſeful;

Lac, or gum lac, as it is popularly but improperly called, is certainly a very uſeful ſubſtance; and the natives of India furniſh full proofs of this, by the many purpoſes to which they apply it.

for rings, beads;

According to Mr. Kerr, it is made by them into rings, beads, and other female ornaments.

When

When formed into ſealing wax, it is employed as a japan, and is likewiſe manufactured into different coloured varniſhes. ſealing wax, varniſhes;

The colouring part is formed into lakes for painters: a ſort of Spaniſh wool for the ladies is alſo prepared with it: and, as a dying material, it is in very general uſe. lakes, dying;

The reſinous part is even employed to form grindſtones, by melting it, and mixing with it about three parts of ſand. For making poliſhing grindſtones, the ſand is ſifted through fine muſlin; but thoſe which are employed by the lapidaries, are formed with powder of corundum, called by them Corune. * grindſtones, and poliſhers;

But, in addition to all the above uſes to which it is applied in India, as well as to thoſe which cauſe it to be in requeſt in Europe, Mr. Wilkin's Hindû ink occupies a conſpicuous place, not merely on account of its uſe as an ink, but becauſe it teaches us to prepare an aqueous ſolution of lac, which probably will be found of very extenſive utility. the Hindû ink;

This ſolution of lac in water may be advantageouſly employed as a ſort of varniſh, which is equal in durability, and other qualities, to thoſe prepared with alcohol; whilſt, by the ſaving of this liquid, it is infinitely cheaper. water varniſh:

I do not mean however to aſſert that it will anſwer equally well in all caſes, but only that it may be employed in many. It will be found likewiſe of great uſe as a vehicle for colours; for, when dry, it is not eaſily affected by damp, or even by water.

With a ſolution of this kind, I have mixed various colours, ſuch as vermillion, fine lake, indigo, Pruſſian blue, ſap green, and gamboge; and it is remarkable, that although the two laſt are of a gummy nature, and the others had been previouſly mixed with gum, (being cakes of the patent water-colours,) yet, when dried upon paper, they could not be removed with a moiſtened ſponge, until the ſurface of the paper itſelf was rubbed off. The latter very uſeful. Inſtances.

In many arts and manufactures, therefore, the ſolutions of lac may be found of much utility; for, like mucilage, they may be diluted with water, and yet, when dry, are little if at all affected by it.†

We

* Phil. Tranſ. 1781, p. 380.

† The alkaline ſolutions of lac are evidently of a ſaponaceous nature, and, like other ſoaps, may be decompoſed by acids. The entire

Reſins acted on by acids and alkalis.

We find, from the experiments on lac, that this ſubſtance is ſoluble in the alkalis, and in ſome of the acids. But this fact (conſidering that reſin is the principal ingredient of lac) is in oppoſition to the generally received opinion of chemiſts, namely, that acids and alkalis do not act upon reſinous bodies. Some experiments, however, which I have made on various reſins, gum-reſins, and balſams, fully eſtabliſh, that theſe ſubſtances are powerfully acted upon by the alkalis, and by ſome of the acids, ſo as to be completely diſſolved, and rendered ſoluble in water.

Field of inquiry.

It will be a very wide and curious field of inquiry, to diſcover what changes are thus produced in theſe bodies, eſpecially by nitric acid. Each ſubſtance muſt form the ſubject of a ſeparate inveſtigation; and there cannot be a doubt but that much will be learned reſpecting their nature and properties, which hitherto have been ſo little examined by chemiſts.

Utility of reſinous ſolutions.

The alkaline ſolutions of reſin may be found uſeful in ſome of the arts; for many colours, eſpecially thoſe which are metallic, when diſſolved in acids, may be precipitated, combined with reſin, by adding the former to the alkaline ſolutions of the latter. I have made ſome experiments of this kind with ſucceſs; and perhaps theſe proceſſes might prove uſeful to dyers and manufacturers of colours. It is probable alſo, that medicine may derive advantages from ſome of this extenſive ſeries of alkaline and acid ſolutions of the reſinous ſubſtances.

entire ſubſtance of lac is not however completely diſſolved, as appears from the turbidneſs of the liquors. Three of the four ingredients, namely, the reſin, the gluten, and the colouring extract, appear to be in perfect ſolution; whilſt the wax is only partially combined with the alkali, and forms that imperfectly ſoluble ſaponaceous compound which has been formerly mentioned, and which remains ſuſpended, and diſturbs the tranſparency of the ſolution.

From various circumſtances, it does not ſeem improbable, that the long ſought-for, but hitherto undiſcovered vehicle employed by the celebrated painters of the Venetian School, may have been ſome kind of reſinous ſolution, prepared by means of borax, or by the alkalis.

A ſimple

VIII.

A ſimple and accurate Method of Surveying on Shore, with ſuch Inſtruments only as are in every one's Poſſeſſion. By Captain J. MORTLOCK. *From the Author.*

To Mr. NICHOLSON.

28, *Surry Place, Kent Road,*
7th January, 1805.

SIR,

Introductory letter.

ALMOST all our treatiſes on nautical ſurveying begin with the explanation and deſcription of what are called the neceſſary inſtruments, which are deſcribed ſo numerous, and the price ſo conſiderable, that very few can procure them: Thus diſcouraged, they abandon every idea of making plans of ſuch ports as they touch at, for want of what they conceive to be the neceſſary inſtruments.

To obviate this difficulty, and to render nautical ſurveying more general, I have, in the annexed paper, attempted to ſhew the mariner how to ſurvey any port or place he may touch at, with great accuracy, little trouble, and without any expence for inſtruments. Should you find this ſimple method deſerving of a place in your valuable Journal, I ſhall feel myſelf honoured by your inſerting it.

I am, Sir,

Your moſt obedient humble ſervant,

J. MORTLOCK.

Nautical ſurveying without inſtruments.

FIRST make an eye-ſketch of the place to be ſurveyed, as the annexed figure, numbering all the points, bays, rocks, ſhoals, &c. Chooſe two ſtations, as A and B *(Fig.* 1, *Plate V)*, whence all the rocks, points, &c. may be ſeen from, and ſo ſituated from each other, that the bearings of the points, &c. as taken from A and B, ſhall interſect at angles at leaſt greater than 10 degrees, but the nearer 90 degrees the better.

Having choſen the ſtations, proceed to one of them as A, and place the paper intended to receive the plan horizontally before you, extended by pins, or otherwiſe, on a board ſecurely fixed, to prevent it ſhifting its poſition while the bearings are drawing.

Stick

Nautical surveying without inſtruments.

Stick a pin through the paper firm into the board, at the part meant to repreſent the ſtation A, and lay a ruler with a perfect ſtraight edge on the paper, touching the pin at A and pointing towards the ſtation B, and draw the line A B: in like manner draw lines from A towards all the points, rocks, bays, &c. numbering the lines as the points, rocks, bays, &c. are numbered in the eye-ſketch: Proceed next to the ſtation B, and place the board horizontally before you, ſo that the line A B ſhall point back towards A, and ſecure the board with the ſame precaution as at A, to prevent its ſhifting: then, in the line A B, ſtick a pin firm through the paper into the board, in that part meant to repreſent the ſtation B; from which point draw lines pointing towards the different points, rocks, &c. as was done from A, numbering them in like manner. Now, where the lines drawn from B interſect thoſe of the ſame number drawn from A, will be the place of the points, rocks, &c. to which the lines were directed to from the ſtations. Sketch in the ſhore between the points, &c. and the plan is completed.

The meridian-line may be found by compaſs, or more correctly, by drawing the line of the ſun's bearing from one of the ſtations, and taking his altitude at the ſame time. Then with the latitude, altitude, and declination, compute the azimuth, and lay it off to the left or right of the line of the ſun's bearing, according as the ſun was to the right or left of the meridian, and it will give the true north and ſouth, or meridian-line.

If the diſtance between any two points on the ſhore be meaſured, it will give you a ſcale for the plan; but it may often be found more convenient to meaſure off a baſe, as A C, from one of the ſtations, in a direction nearly perpendicular to the line A B; and let it be in length equal to ſome part of a geographical mile, as 380 feet $= \frac{1}{16}$, or 760 $= \frac{1}{8}$, or 1520 $= \frac{1}{4}$, or 3040 $= \frac{1}{2}$, or any part of a mile; then will the line A C be a ſcale to the plan.

I have ſuppoſed any common board and ruler to illuſtrate the ſimplicity of this method of ſurveying: but to ſuch as are provided with a drawing frame, it will be found convenient to extend the paper on; and if a ruler has ſights perpendicular to its edge, it will be found commodious, and require leſs trouble. I hope the eaſe and expedition with which the whole

is

is performed, will induce ſea-faring people to amuſe themſelves by taking plans of the places they touch at: for it is by the improvement of geography that the dangers of navigation are diminiſhed, and, conſequently, the lives and property embarked in our ſhipping are leſs expoſed to danger.

J. MORTLOCK.

IX.

Notice of an Omiſſion in ACCUM'S *Chemiſtry, of the direct Production of Nitric Acid. By* W. F. S.

To Mr. NICHOLSON.

SIR, *Lincoln's Inn, January* 7, 1805.

HAVING in your Philoſophical Journal ſpoken very handſomely, and not undeſervingly ſo, of Mr. Accum's Treatiſe on Chemiſtry, it would not perhaps be improper in one of your enſuing Numbers, either to elucidate or correct a ſmall difficulty, or rather error, in that book. In deſcribing the mode of producing nitric acid, in the ſecond volume of Mr. A.'s work, p. 286, he puts as a principal ingredient *nitrate of potaſh:* Now, on referring to the mode of obtaining *nitrate of potaſh* in page 287, and following page, it is deſcribed as being produced by neutralizing the carbonate of potaſh with *nitric acid:* Now, Sir, it is pretty evident, that the means of preparing either nitric acid or nitrate of potaſh is not given, or rather it is ſtated by *implication,* as *incapable of being produced by art.*

Nitre and nitric acid in Accum's chemiſtry.

Very reſpectfully your's,

W. F. C.

P. S. As this error can only be rectified by a ſubſequent edition, it would, with ſubmiſſion, be extremely beneficial to the holders of the preſent edition, that you ſhould introduce the emendation in your Journal.

On

X.

On Galvaniſm. By Mr. CHARLES SYLVESTER.

To Mr. NICHOLSON.

SIR,

Difficulties reſpecting the galvanic decompoſition of water; as to the diſtances of the wires.

YOU did me the honour of inſerting in your valuable Journal, ſome experiments tending to illuſtrate the theory of galvaniſm. I do not know who originally propoſed the idea of the combination of electricity with hydrogen (the truth of which my experiments were intended to eſtabliſh), though I am now bound to acknowledge the ingenuity and importance of the thought. Such an idea would perhaps never have been ſuggeſted, had it not been for the very paradoxical appearances attending the decompoſition of water by galvaniſm. The appearance of the hydrogen at ſo great a diſtance from the oxigen, both of which muſt have been produced from the ſame particle of water, was very ſatisfactorily accounted for by this conjecture. The continental philoſophers, and Dr, Gibbes of Bath, did not ſo well account for the phenomena by their hypotheſis, though it appeared ſo formidable, as to threaten the theory of modern chemiſtry with diſſolution.

Hypotheſis of Mr. Wilkinſon cenſured.

Another hypotheſis of the decompoſition of water, was given by Mr. Wilkinſon, in his Elements of Galvaniſm, and in your 36th Number. If we even allow Mr. W. the advantage of all his very gratuitous data in accounting for the decompoſition of water, the contradiction with which they abound, will totally render his hypotheſis invalid.

Remarks in detail on Mr. W.'s hypotheſis.

Mr. W. begins by ſuppoſing a particle of water analogous to the Leyden phial, which is the ſame thing as to ſuppoſe that water, a conductor of electricity, is compoſed of particles in themſelves non-conductors. Mr. W. is of opinion, that the decompoſition takes place in the middle of the liquid between the points of the wires. The way in which he ſuppoſes the ſeparation to be effected, is ſomething like the idea the ancients had of the ſolutions of metals in acids, *viz.* that the metal was ſplit into very minute particles (capable of being ſuſpended in the liquid) by the wedge-like particles of the acid. After the decompoſition is effected, he tells us that the capacity of the hydrogen is diminiſhed for electricity, while

while the capacity of oxigen is increaſed for the ſame ſubſtance; two facts for which I ſhould be glad to know Mr. W.'s authority. He now ſuppoſes the oxigen to be attracted by the poſitive wire, where it ſaturates itſelf with the electricity neceſſary to conſtitute its gaſeous form. The hydrogen, on the other hand, is attracted to the negative wire, to which it muſt give its exceſs before its elaſtic form can be effected.

Objection.

After all the labour Mr. W. has beſtowed to bring the hydrogen to one wire, and the oxigen to the other, he does not appear, in my opinion, to have ſucceeded; for the exceſs of electricity in the hydrogen would be given to the deficient oxigen, and the gaſſes would be given out in the middle of the liquid, and not at the ends of the wires.

Water not decompoſed in a very ſmall tube.

Mr. W. mentions the curious fact of water not being decompoſed in a very ſmall tube, and alſo, that no decompoſition takes place when the wires in the liquid are as much as eight inches diſtant from each other. I have long been in poſſeſſion of theſe facts: I have varied theſe experiments by uſing, inſtead of pure water, different ſolutions of ſalts, with a view to increaſe the conducting power of the liquid medium: I found a ſolution of carbonate of potaſh to anſwer very well: I placed the wires in the ends of a tube of more than a yard in length: The decompoſition went on very rapidly: I ſoon obſerved the poſitive wire coated with beautiful carbonate of copper, at the ſame time I obſerved a gaſeous ſubſtance diſengaged: I found it, by the teſt of lime-water, to be carbonic acid gas. I obſerved the diſengagement of this gas, after I had rendered the potaſh very cauſtic by lime.

Saline ſolutions are decompoſed.

Carbonate of potaſh.

Pure potaſh.

Common ſalt.

I afterwards uſed a tube of $\frac{1}{20}$th of an inch in diameter, and five feet long, into which I introduced a ſaturated ſolution of common ſalt. After the wires were introduced, and the communication made, I obſerved bubbles of hydrogen upon the negative wire, in about a minute after *.

I remain, Sir,

Your's, &c.

CHARLES SYLVESTER.

Sheffield, Jan. 20, 1805.

* This corroborates the fact of the hydrogen paſſing inviſibly through the liquid.

XI.

Reply to the Animadverſions and Experiments of C. L. *on the Subject of the Horizontal Moon. By* Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

DEAR SIR,

WHAT can have induced your correſpondent C. L. to attempt to confute my paper relating to the horizontal moon, with ſo much invidiouſneſs, is beſt known to himſelf. For my own part, I believe that he is perſonally unknown to me, and that I am equally ſo to him.

Whether the teleſcope be properly urged againſt Mr. W.'s theory.

The firſt ſtep that this faſtidious writer takes to diſprove the truth of my poſition is a falſe one. He ſays, that "if Mr. Walker's poſition were true, the magnifying power of the ſame teleſcope would vary with its aperture." *

C. L. has been very unfortunate in mentioning the teleſcope, for the properties of that inſtrument will confute every argument which he has uſed againſt my theory, and ſhow that his ill conducted experiments, like an *ignis fatuus*, tended only to miſlead him.

The want of a ſtandard would prevent its being ſeen whether a leſs light gives a greater appearance to objects.

I ſhall drop this ſubject for the preſent, to examine his next aſſertion in the ſame paragraph. The apparent magnitude of the paper before the eye may become larger, when the candle is ſhaded with the hand, for any thing that C. L. knows to the contrary, as the increaſe is too ſmall to be perceived by our ſenſes; and even if it were ten times larger than it is, it could not be known, becauſe every other object before the eye would increaſe in apparent magnitude at the ſame time, and in the ſame ratio; and conſequently leave no ſtandard to compare the paper with. To elucidate this in a familiar way, permit me, Sir, to aſk how would C. L. determine the number of miles between Troſton in Suffolk, and Soho-Square in London, without ſome ſtandard meaſure, with which to compare that diſtance.

Strictures on C. L.'s experiments.

Then follow his "correct experiments of the ſame deſcription as mine."—My experiments were made to imitate the eye, upon a large ſcale. Let us ſee how C. L. has imitated nature in his experiments.

* Philoſ. Journal, Vol. IX. p. 235.

In

In his firſt experiment, "from the flame to the lens the diſtance was 49½ inches, and from the lens to the image it was 88 inches." Theſe numbers being reduced to the ſcale of the human eye, it will appear that the diſtance between the cryſtalline lens and the retina, is nearly twice as great as between the cryſtalline lens and the object; conſequently, if we take the diſtance between the cryſtalline and the outward ſurface of the cornea into the calculation, the object muſt nearly touch the eye. This, I think, may be called *a ſhort-ſighted experiment*. Exp. 1. The object was too near the lens.

The concluſion of C. L.'s paper contains a number of particulars, in which he *ſuppoſes* I have erred, and then modeſtly "ſubmits it to yourſelf and readers, whether I have acted conſiſtently with the rules of philoſophical inveſtigation," &c.

But as it will appear hereafter, that none of thoſe errors had any exiſtence, except in C. L.'s own mind, his concluſion can ſerve no other purpoſe than to ſhew the temper and diſpoſition of the writer.

In my paper which was honoured with a place in the 9th vol. of this Journal, page 164, I only gave an abſtract of a ſeries of experiments, on which I founded my general concluſion; but it now appears neceſſary to give a more particular account of them, to ſhew that they are entitled to more credit than C. L. has thought fit to give them. A more particular account of the author's former experiments.

After having prepared my apparatus as deſcribed in my paper above-mentioned, with a long mould candle of ſix to the pound, placed in an inclined poſition, I began with meaſuring the focal image of the whole lens.

The length of this luminous picture upon the ſcreen, was determined by a pair of compaſſes, and a diagonal ſcale of inches. This meaſure was entered on page 1 of a book, prepared for that purpoſe.

The card No. 1. was then placed before the lens; the luminous picture meaſured in the ſame manner as before, was entered on page 2 of the ſame book.

The meaſure of the picture given by the aperture, No. 2. was entered on the 3d page. And the meaſure of that given by No. 3. was noted down on the 4th page.

As ſoon as theſe four experiments were finiſhed, I began again with the whole lens, and entered a ſecond meaſure on page 1. under the firſt, and continued till I had obtained two meaſures

meaſures on each page: and in this manner, I continued my experiments until I had obtained five or ſix meaſures to each aperture. This mode gave me an opportunity of ſeeing how much the flame of the candle altered in its length, during the time that I was making the experiments, which alteration was too ſmall to be regarded. The meaſures on each page being added up, and divided by the number of them, gave me thoſe numbers which were inſerted in my paper.

But I did not ſtop here, for ſets of experiments were repeated in the ſame manner, on ſeveral evenings, to aſcertain the fact more clearly. But to come more immediately to the point; when the ſun or moon was uſed inſtead of a candle, the ſame reſult was obtained, viz. the greateſt aperture gave the greateſt luminous picture.

Principles of optics brought in ſupport of the author's theory.

The truth of this property in optics, however, does not wholly reſt on my experiments, the ſame concluſion may be derived from other principles.

The pencils of light that enter the eye are ſmaller, the ſmaller the aperture of the object lens;

It is a well known property of the teleſcope, that "as the aperture is contracted, the ſlender pencils or cylinders of rays that emerge from the eye-glaſs into the eye, are alſo contracted in the ſame proportion." *

The magnifying power, of the object glaſs of a teleſcope is not increaſed by increaſing its aperture; but by increaſing its aperture, you increaſe the magnitude of the pencil of light in its focus: for as the eye-glaſs remains the ſame, the increaſe in magnitude of the emerging pencil of rays, muſt depend upon an increaſe in magnitude of the pencil in the focus of the object glaſs. Conſequently, when the moon is viewed, if the aperture of the object glaſs be increaſed, the picture of the moon within the teleſcope, will be increaſed in the ſame ratio; but without increaſing the magnifying power of the inſtrument.

—whence it is inferred that the moon's picture will be equally changed in ſize.

For the magnifying power is = the diameter of the aperture of the object glaſs divided by the diameter of the emerging pencil.

Demonſtration.

Let A = the diameter of the aperture of the object glaſs, B = the diameter of the emerging pencil, C = the focal diſ-

* Elementary Parts of Smith's Optics, page 93.

tance

tance of the object glaſs, and D the focal diſtance of the eye-glaſs. Then $\frac{C}{D}$ = the magnifying power.

But C : D : : A : B. (Smith's Optics, p. 93.) And by diviſion $\frac{C}{D} = \frac{A}{B}$. Therefore the enlargement of the picture of an object within the teleſcope, does not increaſe its magnifying power.

Hence theſe properties of the teleſcope prove to a demonſtration, that the concluſion which I have drawn from my experiments is a law of viſion; and this law ſhows us, that no concavity of the ſky; no terreſtrial perſpective, nor even *the painting of the waves under Black-Friar's Bridge*, can explain the phenomenon in queſtion *. The cauſe lies wholly within the eye. Concluſion.

It is a moſt unpleaſant taſk to enter into controverſy, but I cannot ſee how I could avoid it in this inſtance. It appears to me, Sir, that you inſerted C. L.'s paper for me to anſwer, and I have complied; hoping, however, that this is the laſt time of my having occaſion to uſe my pen, againſt an anonymous writer. Common juſtice ſeems to demand the names of thoſe who undertake the examination of ſuch papers as are owned by their reſpective authors. Reſpecting controverſy.

I am, Dear Sir,

Your moſt obedient ſervant,

EZ. WALKER.

Lynn, Jan. 18, 1805.

XII.

Deſcription of a ſimple Inſtrument for making correct Drawings from Nature. By T. C. B.

To Mr. NICHOLSON.

SIR, *January* 22, 1805.

THE deſcription of two inſtruments for facilitating landſcape drawing from nature given in the firſt volume, page 281, of your Journal, has ſuggeſted to me the conſtruction of

* Philoſophical Journal, vol. IX. page 237.

another,

Inſtrument for making correct drawings from nature.

another, which ſeems to unite the accuracy of the firſt of thoſe inſtruments with the ſimplicity and portability of the ſecond. As I conceive it may be of ſome ſervice to thoſe who are in the habit of ſketching landſcape, I ſend you a deſcription of one which I have had made, that you may, if you think proper, publiſh it in your Journal. Let A B, *Fig.* II. *Plate* 5. be a flat rule, ſuppoſe twelve inches long, having at its extremities two arms B D and A F turning upon a joint at B and A; and in each arm a circular joint at C and G: let the length of each arm B D and A F be ten inches: a handle to fix on E, and a thread paſſing through two holes equidiſtant from the handle, making any length, according to the angle under which the view may be beſt ſeen.

To uſe the inſtrument, take the end of the thread in the mouth, and hold the inſtrument upright before the eye, then move either or both of the arms till the points D and F are brought in a line between the eye and any point in the landſcape you may wiſh to delineate; lay the inſtrument upon the drawing paper, and you will have the true ſituation of ſuch part of the ſubject.—Proceed in like manner, taking care always to keep the baſe line in the ſame place, till you get as many points as you require, by which means any landſcape or building may be drawn very expeditiouſly, and with a great degree of accuracy.

To make the inſtrument as portable as poſſible, there is a joint in A B, which the handle covers, and the pieces D G and C F are made of thin braſs, to fold into the pieces A C and B G; ſo that the inſtrument, when folded up, need occupy no more room in the pocket than a ſmall ſpectacle caſe.

It is perhaps unneceſſary to add that this inſtrument may be uſed for the purpoſe of copying, and anſwers the purpoſe of a triangular or quadrangular compaſs.

I am, Sir,

Your moſt obedient and humble ſervant,

T. C. B.

If the rough incloſed ſketch of the inſtrument is not ſufficiently intelligible, Mr. Nicholſon may ſee one at Mr. Frazer's, Optician, in Bond-Street.

Obſervations

XIII.

Obſervations on Baſalt, and on the Tranſition from the vitreous to the ſtony Texture, which occurs in the gradual Refrigeration of melted Baſalt; with ſome geological Remarks. In a Letter from GREGORY WATT, *Eſq. to the Right Hon.* CHARLES GREVILLE, *V. P. R. S. From the Philoſophical Tranſactions for* 1804, *p.* 279.

SIR, *Soho, April* 1804.

THE important geological conſequences that ſeem deducible from the changes of texture developed by Sir James Hall's very judicious experiments on the regulated cooling of melted baſalt, induced me to attempt a repetition of them, ſome time after the publication of his intereſting and ingenious paper. * I believe that formerly I had the honour of ſhowing you ſome of the reſults of my imperfect and diminutive experiments, which only ſerved to afford additional proofs of the tranſition from the vitreous to the ſtony texture, which takes place in the gradual refrigeration of glaſs. Circumſtances have prevented my reſuming theſe inveſtigations, till it lately occurred to me that ſomething might be learned, by expoſing to the action of heat, a much larger maſs of baſaltic matter than, as far as I am informed, had ever at one time been ſubjected to experiment.

Sir James Hall's experiments on the ſlow congelation of fuſed baſalt.

One of the common reverberatory furnaces uſed in iron founderies for the fuſion of pig-iron, was ſtrongly heated by a fire maintained for ſeveral hours. About ſeven hundred weight of amorphous baſalt, here called Rowley Rag, was broken into ſmall pieces, and depoſited gradually on the elevated part of the interior of the furnace, between the fire and the chimney, from whence, as it melted, it flowed into the deeper part, in which, in ordinary operations, the melted iron is collected. It was obſerved by the perſons attending, that it did not require half the quantity of fuel to fuſe the baſalt, that would have been neceſſary to melt an equal weight of pig-iron. When the whole was melted, it formed a liquid glaſs, rather

The experiment repeated on 7 cwt. of rowley rag.

It was eaſily fuſed, and formed perfect glaſs when quickly cooled.

* Publiſhed in the Tranſactions of the Royal Society of Edinburgh, Vol. V. and in our Journal, Vol. V. quarto ſeries.

tenacious, from which a large ladle-full was taken, which, on being allowed to cool, retained the characters of perfect glass. The fire was maintained, though with gradual diminution, for more than six hours; after which time, the draught of the chimney was intercepted, the surface of the glass was covered with heated sand, and the furnace was filled with coals, which were consumed very slowly. It was eight days before the mass in the furnace was sufficiently cool to be extracted, and even then it retained considerable internal heat.

Slow congelation.

Appearance of the mass.

The form of the mass, being given by the bottom of the furnace, was considerably irregular, approaching to the shape of a wedge whose lower angles were rounded. It was nearly three feet and a half long, two feet and a half wide, about four inches thick at one end, and above eighteen inches at the other. From this diversity of thickness, and from the unequal action of the heat of the furnace, too great an irregularity had prevailed in the refrigeration of the glass, to permit its attainment of a homogeneous texture. These circumstances might probably have been counteracted by better devised precautions; but the inequality of the product is not to be regretted, since it has fortuitously disclosed some very singular peculiarities, in the arrangement of bodies passing from a vitreous to a stony state, which might have remained unobserved, if the desired homogeneity of the result had been obtained. I shall now endeavour to describe the various products of this operation; and I shall also submit to your consideration, some remarks which appear to me to arise naturally from the phenomena I have observed; premising that, except where my opinions are supported by the unequivocal demonstration of facts, I offer them with the utmost deference to the decision of more experienced and judicious mineralogists and geologists.

External characters, &c. of rowley rag.

It may be proper to give a concise description of rowley rag itself before I consider the products which it yields by igneous fusion. This species of basalt is fine-grained, of a confused crystallized texture; its fracture uneven in small pieces, conchoidal in large pieces. Its hardness superior to common glass but inferior to feldspar. Its tenacity considerable. Its action on the magnetic needle strong, but without signs of polarity. Its specific gravity, according to my trials, 2.868. Its general colour iron gray, approaching to black. It is opaque; and it

reflects light from a number of brilliant points, ſome of which ſeems to be feldſpar, and the others hornblende. *

1ſt. This ſubſtance is eaſily fuſed into glaſs, whoſe texture is completely vitreous, with few air-bubbles. Its fracture undulated conchoidal. Its hardneſs ſuperior to feldſpar, but inferior to quartz. It poſſeſſes ſcarcely any action on the magnetic needle. Its colour is black: it is nearly opaque, being tranſlucent only in very thin fragments. Its ſpecific gravity appears to be 2.749. Its glaſs.

2d. The tendency towards arrangement in the particles of the fluid glaſs, is firſt developed by the formation of minute globules, which are generally nearly ſpherical, but ſometimes elongated, and which are thickly diſſeminated through the maſs. The colour of theſe globules is conſiderably lighter than that of the glaſs; they are commonly grayiſh-brown, ſometimes inclining to chocolate brown, and, when they have been formed near the interior ſurface of the cavities in the glaſs, they project, and reſemble a cluſter of ſmall ſeeds. Their diameter rarely exceeds a line, and ſeldom attains that ſize, as, in general, they are ſo near to one another, that their ſurfaces touch before they can acquire conſiderable magnitude. In the proceſs of cooling, they adapt their form to their confined ſituation, fill up every interſtice, and finally preſent a First appearance of arrangement. Small globules appear in the maſs.

* "The ragſtone has been accurately analyſed by Dr. Withering, who found that 1000 parts of it contained 475 parts of ſiliceous earth, 325 argillaceous earth, and 200 calx of iron; but this iron ſeems to me to be in a very ſmall degree of calcination, from the dark blue colour of the ſtone, from the ruſty colour it aſſumes on being expoſed to a farther ſtate of calcination by air and water, and from the magnetic property of the mountains, which, as Dr. Plot obſerved, turned the needle 6° from its proper direction. This magnetic property has ſince been obſerved in ſeveral baſaltic mountains, particularly in the Giant's Cauſeway in Ireland, and very remarkably in a baſaltic columnar mountain called Compaſs Hill, in the iſland Cannay, one of the Hebrides, deſcribed by George Dempſter, Eſq. in the Tranſactions of the Society of Antiquaries in Scotland, Vol. I." See Mineralogy of the South-weſt part of Staffordſhire, by James Keir, Eſq. F. R. S. publiſhed in Shaw's Hiſtory of Staffordſhire, Vol. I. Analyſis of rowley rag by Dr. Withering.

Mr. Kirwan ſtates the ſpecific gravity of rowley rag, which he calls *ferrilite*, at 2.748; and aſſigns its melting point at 98° of Wedgwood's pyrometer.

homogeneous body, wholly unlike glaſs, and equally unlike the parent baſalt. When the union of the little globules has been imperfectly effected, the fracture of the maſs indicates its ſtructure, by numerous minute conchoidal fractures, which diſplay the form of each globule. But, if the arrangement has extended a little farther, all theſe ſubdiviſions are entirely loſt; the maſs becomes perfectly compact, has an even or a flat conchoidal fracture, is nearly of the ſame hardneſs as the glaſs, is commonly of a chocolate colour, graduating into a browniſh-black, and the intenſity of the colour increaſes in proportion to the degree to which the arrangement has extended. Its aſpect is rather greaſy; and it much reſembles ſome varieties of jaſper in the compactneſs of its texture, and in its opacity. Its magnetic action is extremely feeble. Its ſpecific gravity appears to be 2.938.

It reſembles jaſper, and has little magnetic action.

3d. If the maſs were now rapidly cooled, it is obvious that the reſult would be the ſubſtance I have juſt deſcribed; but, if the temperature adapted to the farther arrangement of its particles be continued, another change is immediately commenced, by the progreſs of which it acquires a more ſtony texture, much greater tenacity, and its colour deepens as theſe changes advance, till it becomes abſolutely black. Sometimes this alteration is effected by a gradual tranſition, the limits of which cannot be aſſigned, but more generally by the formation of ſecondary ſpheroids, in the heart of the compact jaſpedeous ſubſtance. Theſe ſperoids differ eſſentially from thoſe firſt deſcribed; the centres of their formation are more remote from each other, and their magnitude is proportionably greater, ſometimes extending to a diameter of two inches, and ſeeming only to be limited by contact with the peripheries of other ſperoids. They are radiated, with diſtinct fibres; ſometimes the fibres reſemble thoſe of brown hæmatites, and ſometimes they are faſciculated irregularly, ſo as to be very ſimilar in appearance to the argillaceous iron ores rendered priſmatic by torrefaction. They are generally well defined, and eaſily ſeparable from the maſs they are engaged in; and often the fibres divide at equal diſtances from the centre, ſo as to detach portions of the ſperoid in concentric coats. The tranſverſe fracture of the fibres is compact and fine grained; the colour black; and the hardneſs ſomewhat inferior to that of the baſaltic glaſs. When

More advanced appearance of the ſtony texture.

Darker colour.

Large ſpheroids of radiated texture.

two

two of the ſpheroids come into contact by mutual enlargement, no intermixture of their fibres ſeem to take place; they appear equally impenetrable, and, as neither can penetrate, both are compreſſed, and their limits are defined by a plane, at which a ſeparation readily takes place, and each of the ſides is inveſted with a ruſty colour. When ſeveral ſpheroids come in contact on the ſame level, they are formed by mutual preſſure into pretty regular priſms, whoſe diviſion is perfectly defined; and when, a ſpheroid is ſurrounded on all ſides by others, it is compreſſed into an irregular polyhedron.

Contact with ſpheroids produces polyhedrons.

4th. The tranſition from this fibrous ſtate to a different arrangement, ſeems to be very rapid; for the centre of moſt of the ſperoids becomes compact, before they attain the diameter of half an inch. As the fibrous ſtructure propagates itſelf by radiating into the unarranged maſs, the compact nucleus which ſupplies its place gradually extends, till it finally attains the limits of the ſpheroids; and the ſame arrangement pervades the matter comprehended between them. The maſs has now aſſumed a compact ſtony texture, and poſſeſſes great tenacity. Its hardneſs is ſomewhat inferior to that of the glaſs from which it was formed. Its action on the magnetic needle is very conſiderable. Its ſpecific gravity is 2.933. Its colour is black, inclining to ſteel gray: it is abſolutely opaque, and only reflects light from a few minute points. Though the diviſions between the ſpheroids are rendered imperceptible to the eye, they are not obliterated, and their ruſty ſurfaces are often diſcloſed by an attempt to fracture the maſs.

Third arrangement. Compact ſtony texture, great tenacity, opake, conſiderable magnetic action.

5th. A continuation of the temperature favourable to arrangement, ſpeedily induces another change. The texture of the maſs becomes more granular, its colour rather more gray, and the brilliant points larger and more numerous: nor is it long before theſe brilliant molecules arrange themſelves into regular forms; and, finally, the whole maſs becomes pervaded by thin cryſtalline laminæ, which interſect it in every direction, and form projecting cryſtals in the cavities. The hardneſs of the baſis ſeems to continue nearly the ſame; but the aggregate action of the baſis, and of the imbedded cryſtals, on the magnetic needle, is prodigiouſly increaſed. It appears to poſſeſs ſome polarity; and minute fragments are ſuſpended by a magnet. Its ſpecific gravity is ſomewhat increaſed, as it is now 2.949. The cryſtals contained in it, when

Granular texture.

Cryſtalline laminæ.

Polarity.

Increaſed denſity.

when examined by a microſcope, appear to be faſciculi of ſlender priſms, nearly rectangular, terminated by planes perpendicular to the axis; they are extremely brilliant; their colour is greeniſh-black; they are harder than glaſs, and fuſible at the blow-pipe; they are ſuſpended by the action of a magnet. They are arranged nearly ſide by ſide, but not accumulated in thickneſs, ſo that they preſent the appearance of broad thin laminæ; they croſs one another at all angles, but always on nearly the ſame plane; and the lamina thus formed is often three or four lines long, and from a line to a line and a half broad, but extremely thin.*

It ſeems obvious, that an equalized temperature would have rendered the whole ſimilar to the ſubſtance laſt deſcribed; and it may be fairly inferred, that by a continuance of heat, the minute cryſtals would have been augmented in their dimenſions, by the acceſſion of molecules ſtill engaged in the baſis, or by the union of ſeveral cryſtals, till they acquired ſufficient magnitude for their nature to be abſolutely determined by the uſual modes of inveſtigation. It is probable, however, if ſuch precautions had been taken as might have ſecured this degree of perfection in the ulterior reſult, that the maſs would only have exhibited an uniform aſpect, and that the intereſting initial phenomena would not have been diſcovered.†

There

* It may be obſerved, that the cavities which exiſted in the glaſs are not obliterated during the ſubſequent proceſſes, though their interior ſurfaces undergo ſome change. The minute globules firſt formed often become prominent, and project into the cavities. Theſe minute points are ſoon obliterated by the large curves of the fibrous ſpheroids, which give a mamellated form to the interiors of the cavities; and, when the cryſtals are generated in the maſs, they ſhoot into ſome of the cavities, and line them with their brilliant laminæ.

† In this and the ſucceeding paragraphs, the word molecule is uſed in the ſenſe aſſigned to it by Hauy and Dolomieu, and is underſtood to repreſent the peculiar ſolids, of definite compoſition and invariable form, the accumulation of which, forms the cryſtals of mineral ſubſtances. Such molecules, preſerving their form and their eſſential characteriſtics, may be extracted from moſt cryſtals by mechanical diviſion, and may be ſubdivided as far as our ſenſes can recogniſe them. Though we cannot by mechanical means directly divide them into their elementary particles, we are enabled

to

There are ſome conſiderations which appear to offer a partial explanation of the formation of the globules, and of the radiated ſpheroids. It is well aſcertained that heat is emitted by all bodies, in their change from a gaſeous to a fluid ſtate, and alſo in their change from a fluid to a ſolid ſtate. It is reaſonable to ſuppoſe, that heat may alſo be emitted in thoſe changes of arrangement which affect the internal texture of a body, after it has attained an apparently ſolid ſtate. That a ſucceſſion of ſuch changes does actually take place, appears to me demonſtrated by the appearances I have deſcribed, and by the increaſe of ſpecific gravity, which ſeems to keep pace with the internal changes of the ſubſtance. It would appear, that theſe changes are cauſed by a gradual diminution of temperature, which permits certain laws to induce peculiar arrangements among the particles of the glaſs. When ſeveral of theſe particles enter into this new bond of aſſociation, they muſt form a minute point, from which heat muſt iſſue in every direction. That heat will gradually propagate itſelf, till the temperature of the glaſs is equalized; and then the recurrence of the circumſtances which induced the firſt particles to arrange, will cauſe other particles to arrange alſo, which the attraction of aggregation will diſpoſe round the point firſt formed. A ſecond emiſſion of heat in every direction will take place; the temperature will again be equalized; and again another concentric coat of arranged particles will apply itſelf to the little globule. But, at the time when the central point of this globule was formed, the equality of temperature, in the maſs of glaſs, would probably cauſe a number of ſimilar points to be generated. The formation of each muſt proceed in a ſimilar manner to what I have deſcribed, till their ſurfaces touch, and all the glaſs be converted into the ſame ſubſtance.

Explanation of the formation of the globules and ſpheroids; from the emiſſion of heat during congelation.

Theſe globules are therefore formed of concentric coats, but they are alſo radiated. Every one muſt have remarked

General connection between the radiated ſtructure and formation by concentric coats.

to effect this by chemical ſolution, the only power to which their aggregation yields. It will be evident, from the obſervations that follow, that I am inclined to adopt the ingenious idea of Dolomieu, that many apparently homogeneous rocks are compounds of the minute molecules of ſeveral ſpecies of minerals; and that, where a ſuitable opportunity is given, theſe will develope themſelves by the formation of their peculiar cryſtals.

the

the connexion that almoſt uniformly exiſts, between the radiated ſtructure and the formation by concentric coats. There are few radiated ſubſtances which are not diviſible into concentric fragments; and as few concentric arrangements which are not radiated. Of the firſt, it may be ſufficient to mention hæmatites; of the ſecond, calcareous ſtalactites. The tendency to this union of ſtructure, may perhaps be produced by the radiation of the emitted heat, or moiſture, if the ſolution be aqueous; and the diviſions of the coats will naturally take place at thoſe pauſes in the accumulation of particles, which the momentary emiſſion of heat neceſſarily induced.

Formation of the larger ſpheroids explained.

If this be allowed to explain the formation of the firſt ſeries of globules which conſolidate into the jaſpideous ſubſtance, it will alſo explain the formation of the larger and more diſtinctly radiated ſpheroids, which have been already ſtated to be very eaſily diviſible into concentric fragments. They probably were alſo formed round a central point, by the accumulation of thin coats; and the tendency to radiation, which ſeems almoſt inſeparable from this ſtructure, was perhaps aided by the arrangement induced by the emiſſion of heat from every part of the ſurface of the ſpheroids. This mode of formation has the advantage of explaining their impenetrability. Had they been generated by radii diverging from a centre, their compactneſs muſt have diminiſhed as their diameter increaſed; but, in the ſtructure which I have ſuppoſed, each coat is compoſed of particles ſolidly arranged in immediate contact with each other, leaving no ſpaces for penetration. The ſame progreſs is rigidly obſerved in the extenſion of the compact nucleus, which always occupies the centre of the radiated ſpheroids, and finally extends to their peripheries. It obſerves the concentric diviſions of the radiated part with the greateſt preciſion; and the line of their ſeparation is always perfectly defined. But the ſtate of aggregation into which the ſubſtance has now entered, is ſo perfect as to overcome the operation of the cauſes which formerly induced the fibrous ſtructure, and the maſs remains compact. The only change that the ſubſtance afterwards undergoes, conſiſts in the gradual accumulation of the cryſtalline molecules, and their arrangement, by their individual polarity, into regular ſolids. This depends on very different laws from

thoſe

thoſe which conſolidated the fluid glaſs, and aggregated its particles into a compact uniform ſtone.*

Other conſiderations. Remarkable circumſtance, that theſe changes ſucceed after fluidity may be ſuppoſed to have ceaſed, &c.

The appearances that I have endeavoured to deſcribe, ſeem deſerving of conſideration in ſeveral points of view. Few things can be more at variance with commonly received opinion, than the diverſified ſucceſſion of changes of ſtructure which this glaſs exhibits in its paſſage to a cryſtallized ſtate. The generation of the globules which unite to form the jaſpideous ſubſtance, is what we might be prepared to expect, by obſerving the cooling of a common iron furnace flag. But it appears not very obvious to common apprehenſion, that the ſpecies of arrangement requiſite to form this intermediary ſubſtance, could be compatible with any fluidity permitting farther motion of the molecules of the maſs; yet, immediately after the completion of this arrangement, they receive a new diſpoſition, and the radiated fibrous ſtructure commences. Sometimes this pervades even the unaltered glaſs; but I preſume this only to happen where the minute globules firſt formed were ſcattered ſo far aſunder, that their centres became fibrous, before their peripheries came into contact. This view of the ſubject is juſtified by the analogous operation of

* The caſe is conſiderably different, where cryſtals poſſeſſing regular forms are generated in glaſs. The molecules of which they are formed, have doubtleſs been only ſuſpended in the vitreous medium; and their union is determined by cryſtalline polarity, which appears to me perfectly diſtinct from the ſimple aggregation which changes a fluid into a ſolid, whether it be homogeneous or compound, which affects the internal arrangement of thoſe bodies, but which never can ſeparate their components into diſtinct maſſes, or form them into regular ſolids. Every molecule, at the moment of its formation, muſt neceſſarily be endowed with all the properties it afterwards poſſeſſes. The ſuſpenſion of ſuch molecules in a fluid medium, though it may conceal, cannot alter thoſe properties; and the union of ſuch molecules, to form a regular ſolid, in no reſpect alters their individual or aggregate qualities. Whether heat be evolved at the moment of this union, is a queſtion not eaſily ſolved; as the cryſtallizations with which we are familiar are from chemical ſolutions, in which ſome of the molecules are generated by the ſeparation of a combined ſubſtance, at the moment when others are united by cryſtalline polarity.

the

the formation of cryſtals, ſimilar to thoſe deſcribed, in the heart of the radiated ſperoids, while their exteriors ſtill retained the fibrous texture.

— Review of the facts under this point of view.

If it be conſidered as extraordinary, that a change ſhould be effected, converting an apparently ſolid and homogeneous maſs into an accumulation of radiated ſpheroids, and that theſe radii ſhould loſe their fibrous ſtructure, and aſſume the texture, aſpect, and tenacity, of a compact, hard, and homogeneous ſtone, it is certainly much more extraordinary, that this ſtone ſhould permit farther arrangement to proceed, and ſhould enable the cryſtalline molecules which it contains in a ſtate of confuſed aggregation, to arrange themſelves, and to form cryſtals which, although minute, are equal in the perfection of their forms, and in the brilliancy of their natural poliſh, to the moſt precious products of cryſtallization. It is alſo well deſerving of obſervation, by how regular a march the magnetic influence of the ſubſtance keeps pace with the perfection of its arrangement, till it becomes ſo powerful, that fragments of the regenerated ſtone are ſuſpended by the attraction of a magnet.

The act of cryſtallization is incompatible with ſolution or fluid combination.

It has been moſt juſtly remarked by Mr. Smithſon, that ſolution, far from being neceſſary to cryſtallization, effectually prevents its commencement; for, while ſolution ſubſiſts, cryſtallization cannot take place. It may remain a queſtion, whether previous ſolution be eſſential, as a preparatory means of obtaining, by ſubſequent evaporation, or cooling, the ſmall parts of bodies diſengaged, ſo that they may unite to form regular cryſtals. If by ſolution be only meant, that ſimple action of heat, or water, which merely counteracts the force of aggregation, and relieves the molecules from their bonds of union with each other, it certainly is a requiſite; but if by ſolution be meant, that action of affinities by which not only the force of aggregation is overcome, but the combinations which conſtitute the molecules are deſtroyed, it obviouſly is not only unneceſſary but prejudicial to cryſtallization; as a new ſet of molecules muſt be formed, by a new combination of the elementary particles, before the formation of regular bodies can commence.

Suſpenſion is neceſſary; but this may be called mechanical.

The ſuſpenſion of the molecules ready to cryſtallize, may be correctly ſaid to be merely mechanical. Though the mechanical action of trituration can never be expected to reſolve even the moſt eaſily diviſible body in its molecules, becauſe the

the fractures will be at leaſt as frequently acroſs the natural joints as in their direction, yet, even by this rude method, ſome perfect molecules may be diſſengaged; for we find, that water paſſing over large ſurfaces of ſiliceous ſand, finds ſome molecules of ſilex in the ſtate proper for aggregation, and even for cryſtallization. Mechanical ſuſpenſion in a fluid medium, of ſuch denſity that the cryſtalline polarity may be enabled to counteract the power of gravity, is with juſtice conſidered by Mr. Smithſon the only requiſite for the formation of cryſtals. * The circumſtances I have detailed, appear to me an additional confirmation of this remark, and perhaps go ſtill farther, by ſhowing that even the fluidity (in the common ſenſe of the word) of the ſuſpending medium is not an indiſpenſable condition. For it appears impoſſible to annex the idea of fluidity to the union of the minute globules which form the jaſpideous ſubſtance, ſtill leſs to that ſubſtance when formed, and ſtill leſs to thoſe ſpheroids whoſe obſtinate impenetrability is ſo ſtrongly defined. And if, by any power of imagination, theſe can be ſuppoſed to be fluid at the time they retain this conformation, how can it be ſuppoſed that the compact hard tenacious ſtone into which they are changed could retain theſe characters in a fluid ſtate? Yet the ſubſequent formation of cryſtals proves, that either all theſe contradictions muſt be, or that the particles of bodies apparently ſolid muſt be capable of ſome internal motion, enabling them to arrange themſelves according to polarity, while they are ſolid and fixed, as far as they have reference to the ordinary characters of fluidity.

Even fluidity does not ſeem an indiſpenſable condition.

Inſtances even more remarkable have very long been known and authenticated, though perhaps they have not been generally regarded with the attention then deſerve. Glaſs veſſels are well known to be convertible into Reaumur's porcelain, by the internal arrangement of their particles, without loſing their external form, and conſequently at a temperature very much below that requiſite for their fuſion, The change of glaſs into Reaumur's porcelain, does not ariſe from an evaporation of the alkali, as has been alledged, but from a regular arrange-

Inſtances of the internal motions and arrangements of particles of bodies at temperatures below fuſion.

Reaumur's porcelain fully deſcribed:

* See a chemical Analyſis of ſome Calamines, by James Smithſon, Eſq. Philoſophical Tranſactions for 1803, page 27. See alſo Dolomieu, *Journal des Mines*, No. 22, page 53.

ment

ment of the molecules of the glaſs. It commences by the formation of fibres perpendicular to the ſurface of the glaſs, and penetrating into it. At nearly the ſame time, ſmall radiated globules are formed in the interior of the glaſs, and the union of theſe with the fibres, by their mutual increaſe, forms the whole into a new ſubſtance; and, if the requiſite temperature be longer maintained, the fibres diſappear, and the whole becomes fine-grained, and almoſt compact. This ſubſtance, from the improved ſtate of its aggregation, is much ſtronger and more tenacious than before, and is not fuſible at a heat ſufficient to fuſe the glaſs it was formed from; but, if that aggregation be once deſtroyed, the glaſs reſulting from its fuſion is equally fuſible with the original glaſs; and a repetition of the proceſs will again form Reaumur's porcelain, which may be again fuſed, and ſo on repeatedly, for the quantity of alkali evaporated during the operation is extremely ſmall. The hardneſs and brittleneſs of metals rapidly cooled, contraſted with the ſoftneſs and tenacity reſulting from their gradual refrigeration, are all analagous inſtances; and all the proceſſes in which annealing is employed, and more remarkably the tempering of ſteel, the proofs of the internal motions and arrangements of the particles of matter, at temperatures very much below the heat is requiſite for their fluidity.

hardening; tempering; annealing:

Whatever doubts may ariſe reſpecting the formation of the cryſtals, there ſeems no reaſon to ſuppoſe that their gradual increaſe would ceaſe, till all the molecules belonging to that ſpecies were exhauſted, if the temperature favourable to their generation was continued. If the maſs was entirely compoſed of one ſpecies of molecules, it would be reſolved into an aggregation of cryſtals of the ſame ſubſtance; and probably by a ſtill farther continuation of the proceſs of arrangement, into one cryſtal, which, though it might not poſſeſs a regular external form, would be perfect in its internal ſtructure.

Cryſtallization of a maſs of the ſame nature throughout;

But, if the maſs contains two diſtinct ſpecies of molecules, different reſults muſt take place, which will be modified by the proportional quantities of the components. As it has been demonſtrated by Berthollet, that the attraction of maſſes of matter are relatively as their quantities, it follows, that unleſs a very potent counteracting cauſe be exerted, the moſt abundant ingredient in the mixture will be the firſt to cryſtallize.

of particles differing from each other.

Generally, the moſt abundant ingredient will cryſtallize firſt.

But this cryſtallization will not comprehend the whole of its molecules; for, after a certain quantity of them are arranged, the proportions of the remaining fluid are altered; that ingredient which was before the leaſt, may now be equal, or even greateſt, and it will exerciſe its attraction. As the firſt cryſtallization, by ſubtracting a large portion of the fluid particles, muſt have obliged the molecules of the leſs abundant ſubſtance to approach each other very cloſely, they may be able to collect themſelves entirely in their firſt attempt to cryſtallize, or they may form alternate cryſtallizations with the remaining unarranged molecules of the more abundant ſubſtance. However various the ſpecies of molecules may be, they will be regulated by analogous laws, and only ſerve to diverſify the generated ſubſtances. Diverſities.

It by no means follows, that the cryſtals afterwards found to be moſt infuſible would be firſt generated. Their formation does not altogether depend on their greater or leſs fuſibility, but on the relative ſtrength of the attraction which unites them to the matter they are immerſed in, and of the polarity which invites them to cryſtallize. In all cryſtallization from compound fluids, the order in which the ſeveral bodies cryſtallize muſt be determined by their relative quantities and attractions. It is perfectly obvious, that no molecules can form a cryſtal in a heat ſufficient for its fuſion; but it by no means enſues, that it will be formed as ſoon as the molecules are cooled to the point where the cryſtalline polarity overcomes the diſintegrating power of heat; for they may remain ſuſpended in a fluid formed by more fuſible bodies, provided this fluid be ſufficiently abundant to keep them from contact with each other, for the cryſtalline polarity appears to exert itſelf only at extremely ſmall diſtances. In a maſs compoſed of ſubſtances in a ſtate of fluidity, with refractory molecules ſuſpended among them, it is pretty clear, from the preceding paragraph, that the moſt abundant ingredient will be the firſt to cryſtallize. But the removal of a portion of the ſuſpending fluid muſt bring the refractory molecules nearer together, and perhaps ſo near that the cryſtalline polarity may overcome the attraction of the fluid for them; they will therefore cryſtallize next, and will be followed by the remaining ingredients, in the order their attractions dictate. The moſt infuſible cryſtals would not neceſſarily be formed firſt.

As

When refractory ſubſtances cryſtallize after more fuſible matters, they may have the impreſſion of theſe laſt.

As the cryſtals laſt formed muſt neceſſarily be impreſſed, at the parts in contact, by the peculiar forms of thoſe which have been firſt generated, it alſo follows, if the preceding reaſoning be juſt, that the infuſible cryſtals may be found impreſſed by the more fuſible ſubſtance, which cryſtallized firſt; and the remaining ingredients of the mixture, which were ſubſequently arranged, may be moulded on the refractory cryſtals; and thus, in the ſame ſpecimen, may exiſt a refractory ſubſtance generated by fire, impreſſed by more fuſible bodies, and impreſſing them in its turn. From the ſame conſideration it is obvious, that no cryſtal can be formed at a temperature above the degree of its fuſibility; and that, as a neceſſary conſequence, no cryſtal which is more fuſible than the baſis in which it is imbedded, can be formed by igneous operation.

No cryſtal more fuſible than its enveloping maſs, can be formed by igneous operation.

Aqueous ſolution and ſuſpenſion do not differ from thoſe in the dry way. They are effected by the fuſion of the ſolid water, or ice, &c.

The ſame laws muſt regulate the arrangement of aqueous ſolutions, and of molecules ſuſpended in aqueous ſolutions. All theſe are dependant on heat; for we are unacquainted with any fluidity, and conſequently with any ſolution, which heat does not produce. Ice and ſoda have no more action on each other than ſoda and quartz: raiſe the temperature of the ice, and it unites with the ſoda; raiſe the temperature of the ſoda, and it unites with the quartz. Both ſolutions are effected by heat, of the degrees of which we know neither the beginning nor the end, and are therefore utterly unable to eſtimate what aliquot part of its ſcale is adequate to the production of theſe effects. Probably a very minute one.

(To be continued.)

XIV.

Account of the Method of bleaching Cotton, as practiſed at Salzburgh; and the Art of giving a permanent Red to Cotton and Linen. *By* M. C. SCHOERBING.*

Salzburg method of bleaching cotton thread, &c.

COTTON thread is always waſhed before it goes to the weaver. The method of waſhing here to be deſcribed gives it a much whiter colour than ordinary; and it is equally appli-

* Journal of Van Mons, No. 15.

cable

cable to cottons in the piece. This method is called, for what reaſon I know not, the Salzburgh method, though it is chiefly practiſed at Reginſburgh, where four families have long kept it a ſecret, which has not till the preſent occaſion been divulged. This waſhing renders the cotton more flexible; the goods made from it are of a cloſer and more even texture; the cotton does not require any ſubſequent bleaching, and when it comes afterwards to be dyed, it takes a finer, more permanent and more uniform colour. The operation is performed with ſoap, water and ſtrong leys, in the following manner:

Salzburgh method of bleaching cotton thread, &c.

The articles to be waſhed, which uſually conſiſt of thread, ſtockings, night caps, and handkerchiefs, are evenly diſpoſed in a large veſſel or tub. The bottom of this veſſel is firſt covered with a coarſe linen cloth, upon which are laid firſt the handkerchiefs, afterwards the ſtockings and night-caps, and laſtly the thread. The whole is then covered with a ſecond coarſe cloth of a cloſe texture, and very clean, which defends the maſs from the contact of the air. The veſſel itſelf reſembles thoſe uſed for lixiviation in the ſoap work of Konſholm. A boiler having its diameter at the top $1\frac{1}{2}$ Swediſh ell (forty inches) and at the bottom $1\frac{1}{4}$ ell (34 inches) and its depth one ell (27 inches) is filled with water to which a ſufficient quantity of cauſtic leys of potaſh is added to make the fluid produce a greaſy feel between the fingers, and afterwards 2lbs. of ſliced ſoap of Rigenſburg. This liquid is made to boil and poured upon the cotton; a ſhort time after which it is drawn off, to be again heated, during which interval a hot ſolution of ſoap is continually poured upon the cotton. Theſe operations are repeated for four or ſix hours, or until the cotton is well ſoaked, and very hot. It is then left to ſteep for twenty-four hours in ſoap-water. This proceſs is uſually performed between midnight and ſix in the morning; and the next morning the fluid is drawn off, the cotton well waſhed and the water preſſed out by a ſcrew-preſs. This conſtitutes the firſt waſhing.

After the cotton has been well cleanſed in this manner, the principal waſhing is effected as follows:

The waſhed cotton is diſpoſed in the veſſel as before, with this difference only, that the maſs is enveloped in cotton cloth inſtead of linen, which is leſs capable of reſiſting the action of the cauſtic leys. A clear and colourleſs ley is prepared with

two

Salzburg method of bleaching cotton thread, &c.

two thirds wood aſhes and one third lime, and concentrated until an egg deſcends ſlowly in it (ſpecific gravity about 1.081). This ley in a boiling ſtate is poured upon the cotton; after which it is drawn off, again heated, and again poured on; and this repetition is continued from midnight to noon, or for twelve hours. The whole of the ley is then drawn off, and the cottont aken out of the veſſel while hot, with the hands defended by gloves, and after ſpreading it upon a table, it is again put into the veſſel, but in a reverſed order. The ſame waſhing is repeated with other leys and continued for twenty-four hours.

Laſtly, the boiler is filled with water, to which 2lbs. of ſoap are added, and the liquid brought to the boiling heat. The cotton is waſhed with this water, conſtantly kept boiling for twenty hours, and is afterwards left to ſteep for ten or eleven hours. The fluid is then drawn off, the cotton taken out of the veſſel, and ſpread on a long wooden table, where it is waſhed and beetled, and afterwards waſhed in a running water. Laſtly, the water is preſſed out by means of a preſs, and the cotton dried upon ſtaves in the ſun, or in damp weather in a room appropriated to that purpoſe.

The remaining leys after theſe waſhings cannot be uſed a ſecond time, but have a blood-red colour; they are mixed together, and are uſed in the common operations of the laundry. The ſoap waters are thrown away.

The ſtaves on which the cotton is ſuſpended muſt be waſhed every time. It is preferable, when the weather permits, to ſuſpend each piece by threads and ſtretched cords in the open air. The pieces ought not to be brought too near together, for fear leſt the places to which the air has not free acceſs ſhould be ſpotted with brown ſpots.

Piece goods and raw linen thread will receive a partial bleaching by this method. It is neceſſary however to make the leys of only half the ſtrength, and the lixiviation muſt be continued only half the time, for fear of weakening the texture.

Linen and cotton as vegetable ſubſtances have the defect of not taking fixed colours. The cauſe reſides in their reſinous principle, of which they muſt be deprived. Alcohol would be a ſure and eaſy ſolvent for this principle, but it is too expenſive. After alcohol, the beſt ſolvents of this principle are oils

oils of every deſcription, but particularly fiſh oil. The cotton is ſoaked in this oil for 24 hours, after having again boiled it for two hours; it is then run and ſuſpended in the air for ſome days, in order that the oil may be well ſeparated, and it is entirely deprived of the oil by a ſtrong hot ley with ſubſequent waſhing and drying. For this effect a ley of two thirds aſhes and one third lime may be uſed, with the ſame quantity of ſheeps dung as of both the before-mentioned ingredients. The cotton is not only boiled in this mixture, but it is alſo applied ſeveral ſucceſſive times; the goods being waſhed in clear water between each lixiviation. The cotton thread may alſo be waſhed in water containing a ſufficient quantity of potaſh to render it greaſy to the feel. The thread is to be boiled for twelve hours in this ley, or till the fluid becomes black. Laſtly, the thread is waſhed and ſteeped in water acidulated with weakly ſulphuric acid, it is to be taken out of this fluid after remaining in it at leaſt an hour, and then waſhed and dried.

If this method of bleaching were once generally known, it would not be neceſſary to purchaſe the article of the bleacher, or to ſend cotton to them to be bleached. In order to aſcertain whether cotton bleached in any manner whatever is perfectly deprived of its reſin, and proper to receive the dye, the glaſs is to be filled with water, and a thread of the cotton placed on the edge of the glaſs, ſo that half its length ſhall be within and half without. If the former end ſinks in the water and the whole thread acts as a ſyphon, by cauſing it to run over, it will be a proof that the cotton will have the requiſite purity.

Red dye for cottons.

The cotton thus purified muſt paſs through three mordants: 1. A decoction of nut-galls. 2. A ſolution of tin; and 3. alum water.

For the decoction of nut-gall. For 1lb. of cotton or linen thread. For the former half a pound of the dark coloured nut-gall, or for the latter 1lb. is taken. The nut-gall is groſsly pounded and boiled with a handful of birch leaves in a copper boiler, with two and half pots of rain water till reduced to one half. The liquid being left to ſettle and poured while yet hot through a cloth on the cotton, this is ſuffered to ſteep for 24 hours, frequently working or preſſing, in order that it may be univerſally penetrated with the galling principle. It is then

Red dye for cottons.

taken out, rung, and dried by ſuſpending it in the open air, or in a heated chamber. It is needful only to take care that no water ſhall drop upon the cotton, as that would produce ſpots in the dye. In this operation the thread muſt be cloſely packed together, in order that the nut-gall may uniformly penetrate its maſs. It is then conveyed into a ſolution of tin, made as hereafter deſcribed, and the decoction of nut-gall muſt be kept to be uſed in the aluming. The ſolution of tin is prepared in the following manner: Sal ammoniac or ſea ſalt in fine powder is to be diſſolved to ſaturation in 1lb. of aqua-fortis; 2 oz. of fine Engliſh tin raſped, are added to this, or as much of the metal as the acid can diſſolve to ſaturation. In another veſſel, 2 oz. of ſea-ſalt are to be diſſolved in one pint of rain water, and the ſolution of tin is to be poured drop by drop with continual agitation into this ſalt water. The galled thread is afterwards put into a ſtone ware veſſel, cloſely preſſed together, and the laſt mentioned mixture poured thereon. The thread muſt be occaſionally compreſſed with the hand, in order to aſſiſt the penetration, and it muſt then be covered from the air and left to ſteep for twenty-four hours, at the end of which time it is taken out, rung and dried in the air or in a heated room, where it may remain for 48 hours. After this it is waſhed in pure water again, dried, and emerſed in the alum-water, of which we ſhall proceed to deſcribe the compoſition.

The remaining ſolution of tin is reſerved for a ſubſequent operation, for which, in that caſe, no more than three quarters of a pound of aqua-fortis and the other ingredients in proportion, need be taken for 1lb. of thread.

The alum water is made as follows: Whatever may be the kind of alum, it muſt be previouſly calcined. One pound of the crude alum is required for 1lb. of thread. When it is calcined it is pulverized and diſſolved in one pint or Engliſh quart of water, and rather more than an equal meaſure of the remaining decoction of nut-gall is added. Theſe being well mixed and heated, are to be poured on the thread, which has been treated as before with the ſolution of tin. It is left for fourteen hours in this bath and afterwards preſſed and dyed.

The dying 1lb. of good bruiſed or ground madder previouſly ſoaked for ſome hours in water, is put into a boiler of ſufficient ſize, which is to be filled with water and placed on

a moderate fire. As ſoon as the contents begin to heat, the thread is put in and continually turned. It is requiſite that the thread ſhould have been previouſly ſteeped in water, and afterwards wrung, in order that it may be very equally penetrated with the colour. The thread is left in the bath till the liquor boils, after which it is taken out, well waſhed, the threads are arranged on the hand, and the water preſſed out. This firſt dyeing gives the thread a pale red colour. The boiler is then emptied and again filled with water, into which another pound of madder previouſly ſoaked in water is put, and this bath is heated. As ſoon as it has become hot, the thread is put in and boiled for one quarter of an hour. Care muſt be taken that the heat be not too ſtrong. The firſt bath ſhould ſcarcely boil, for fear the articles ſhould take a brown colour; but this inconvenience is not to be feared in the ſecond bath, when the thread is already penetrated with a red colour. After the thread is well waſhed for the ſecond time, it ought to have a deep brown colour; if it has not it muſt be again boiled with a quarter of a pound of madder. After the laſt dye, the thread is to be waſhed by hand in hot ſoap water, after having waſhed it in cold water, and it is then to be dried.

Red dye for cottons.

XV.

Report on an artificial Production of Camphor, announced by M. KIND. *Read to the Society of Pharmacy by* BOULLAY.*

BEING charged, in conjunction with M. M. *Cluzel* and *Chomet*, to give an account to the Society of an intereſting phenomenon announced by *M. Kind*, in *Trommſdorff's Journal de Pharmacie*, extracted and reported in the 14th number of the *Journal de Phyſique et de Chimie*, publiſhed by *Van Mons*, I now inform them of the reſult of our labours.

" For the purpoſe of procuring the arthritic liquor of *Pott*, which is preſcribed here with advantage, for reſolving the nodoſities and other obſtinate tumours which form upon the joints, I intended," ſays *M. Kind*, " to incorporate the muriatic gas with

M. Kind's experiment of the production of camphor by common oil of turpentine to abſorb muriatic gas.

* From Annales de Chimie, Fructidor, No. 153. Vol. LI.

with oil of turpentine, at the inſtant of the diſengagement of the gas. This operation ſhewed me a phenomenon which, I am of opinion, is deſerving of being known.

"I put decrepitated muriate of ſoda into a tubulated retort; I adapted two of Woulf's bottles to the retort, into which I had put a quantity of oil of turpentine, equal in weight to the ſalt. On this I poured half the proportion of concentrated ſulphuric acid, and I diſengaged the muriatic gas by a graduated fire. At firſt the oil acquired a yellow colour, afterwards a pale brown colour, which, towards the end turned to a deep brown. After cooling, the liquor was almoſt wholly coagulated into a cryſtalline maſs, which, in every reſpect, comported itſelf like camphor.

"I do not think," ſays M. Kind, "that a ſimilar production of camphor has ever been noticed before; but Meyer ſpeaks of a concretion of a camphoric nature, formed in oil of turpentine digeſted with dry cauſtic ſalt." He afterwards inſtances a formation of camphor obſerved by the late M. Trommſdorff, on diſtilling eſſential oils on lime. "It is probable," ſays M. Kind, "that the great tendency of muriatic gas to combine with water, determined the union of the principles of this liquid contained in the oil, ſo that the carbon became predominant, and made the oil brown; and, that the effect of this acid gas on it, correſponded with that obſerved by Achard, in the action of concentrated ſulphuric acid on fat oils. He did not remark that the heat was as conſiderable during the action of the gas as Woulf ſtated it to be."

Repeated by M. Trommſdorff.

In an additional note, M. Trommſdorff announces that he had repeated M. Kind's experiment, and that he had obtained analogous reſults.

He dried the concrete matter between ſheets of paper; it became very white, ſemi-tranſparent, and poſſeſſed the following properties:

"Its odour was ſtrong, very analogous to that of camphor, but injured by that of the turpentine.

"Placed in a ſilver ſpoon, and heated over red hot coals, it volatilized without leaving any reſidue, and yielding a ſtrong ſmell of camphor.

"Its vapour inflamed.

"Expoſed to a gentle heat, in a ſmall phial, it was entirely ſublimed. It diſſolved with facility in oil of almonds.

It

"It was entirely diſſolved by alcohol, but not ſo ſlowly as common camphor, and was precipitated unaltered by water.

"Concentrated nitric acid affected its ſolution, at firſt tranquilly, but afterwards with a diſengagement of nitrous gas and heat. The mixture became turbid by the addition of water, but did not depoſit camphor."

He concludes from theſe experiments that this product comported itſelf exactly like camphor, except with the nitric acid; but he believes that this difference may depend on a ſmall quantity of muriatic acid, or a little oil of turpentine, from which it is very difficult to free it entirely.

M. Trommſdorff afterwards diſengaged the gas from a quantity of muriate of ſoda equal to that employed in the firſt experiment, through the matter remaining in the bottle; but he found that inſtead of augmenting, the camphorized part diminiſhed in quantity, loſt its camphoric odour, and the alteration of its other properties induced this chemiſt to believe that the new addition of gas had cauſed it to undergo a decompoſition.

Repetition of theſe experiments by the reports.

The above experiments, repeated with the greateſt attention, gave us ſatisfactory reſults, even with the different ſpecies of oil of turpentine of commerce.

The importance of the ſubject, and the objections made by ſome members, at the reading of the obſervation which lead to this report, made us ſenſible how much the labours of M. Kind left to be wiſhed for; the quantities of the product alſo not being indicated, we thought we ſhould accompliſh the intention of the Society, in following the track marked out by the author, if we endeavoured to add ſome facts to thoſe he has publiſhed, and we propoſed the following queſtions for our guidance.

Queſtions for inveſtigation.

1ſt. Does the gazeous muriatic acid ſerve to determine the re-action of the principles of the oil of turpentine, and to change their proportion, to form the camphor; or, is its action limited to inſulating this immediate principle, from the oil with which it was not combined?

2nd. What is the quantity of the camphoric product?

3rd. Will a greater quantity of the gas than that indicated by M. Kind add to the product, or will it deſtroy it?

4th. Will another quantity of gas added to the liquor which has yielded the camphor, and from which it has been ſeparated, determine a new formation of this principle in it?

5th. What

5th. What are the beſt means of purifying this camphor, and of freeing it from the odour which it retains?

6th. What are the relations and differences between the purified camphor of turpentine, and that of the camphor-laurel?

7th. Would liquid muriatic acid produce the ſame effect?

8th. Would the other acids act in the ſame manner on this oily ſubſtance?

The complete ſolution of theſe different queſtions would doubtleſs have required a great number of experiments, to which we were unable to attend: we ſhall only relate thoſe which we made.

Apparatus. With a view to obtain a more conſiderable product, which would yield us the concrete matter in a quantity ſufficient to be aſcertained and ſubjected to different experiments, we prepared an apparatus ſimilar to that uſed by the author of the diſcovery, but of larger dimenſions. It conſiſted of a tubulated retort, and two Woulf's bottles, provided with their tubes of communication and ſafety. Four pounds of marine ſalt, and two pounds of concentrated ſulphuric acid were introduced into the retort; four pounds of very white oil of turpentine were poured into the firſt bottle, and two pounds of diſtilled water into the ſecond. Fire was placed under the mixture, and augmented gradually till the entire diſengagement of the muriatic acid gas, which was totally abſorbed by the oil. The latter exhibited the following phenomena: it became at firſt of a citron colour, and afterwards browniſh, which grew gradually darker towards the end of the operation; it retained its tranſparency; it grew very hot; its volume augmented about a tenth; the ſides of the bottle were covered with ſmall limpid drops which united ſlowly at its bottom, in the form of a thick oil.

Proportion of ingredients.

All the gas was abſorbed.

Changes of the oil.

Cryſtalline product by repoſe. Left to itſelf for twenty-four hours, it formed a maſs of irregular cryſtals, from which ran a brown liquid. When dried and ſtrongly preſſed between ſheets of paper, this cryſtalline matter was very white, and of a peculiar odour, participating of the oil and the acid made uſe of; it weighed twenty-four ounces.

More cryſtals by cold. The liquor which had been ſeparated from theſe cryſtals, expoſed in a cellar for ſeveral days, yielded others, which dried like the firſt, were of equal whiteneſs, and weighed four ounces.

To

To aſcertain whether there did not yet exiſt a further quantity of this concrete matter ready formed, in this ſort of mother-water of camphor, it was expoſed to an artificial cold of eight or ten degrees below 0, during ſeveral hours. By this means it yielded other cryſtals, which, collected like the preceding, offered the ſame characters, and weighed two ounces; this brings the quantity of camphor to ſeven ounces and a half for each pound of oil of turpentine.

Seven and half oz. of camphor from one pound of oil of turpentine.

This liquor, not appearing capable of yielding more cryſtals, was divided into two portions, one of which was reſerved for examination in this ſtate, the other was ſubjected to the action of a new quantity of acid gas; it diſſolved a part of it, and became almoſt black, without loſing its tranſparence, even after remaining for ſeveral days in the cellar.

More gas did not produce cryſtals in the reſidual liquor.

Another doſe of oil of turpentine was charged with a proportion of gas, double that mentioned above. The precipitation of camphor took place in nearly a ſimilar quantity; the liquor was very little more coloured, but a much greater quantity of fat oil was formed.

Double quantity of gas.

An ounce of this matter waſhed in diſtilled water, and put in a filter to drain, became of the moſt beautiful white; it no longer gave ſigns of acidity, but had ſtill an odour of turpentine. The water in which it had been waſhed was very aromatic; it reddened the tincture of turnſole, and precipitated by nitrate of ſilver.

Action of diſtilled water on this camphor;

An equal quantity of the ſame ſubſtance was waſhed with water impregnated with a fiftieth part of unſaturated carbonate of potaſh: it loſt much of its firſt odour.

and of water acidulated with carbonate of potaſh.

New portions of this rough camphor were mixed in equal parts, one with charcoal in powder, another with very dry aſhes, a third with quick-lime, a fourth with alumine *(porcelain earth)*, and were introduced ſeparately into ſmall glaſs alembics, the capitals of which were, by a gentle heat, rapidly covered with diſtinct groupes, compoſed of a multitude of ſmall needle-formed cryſtals, diverging from the ſame center.

Action of charcoal, aſhes, quick-lime, and alumine.

Purified in this manner, it entirely loſt the odour of oil of turpentine; that which it retained was more analogous to common camphor, but not ſo ſtrong. In this ſtate it ſwims on water, to which it communicates its taſte; it burns on its ſurface, without experiencing the revolving motion obſerved by Profeſſor

Other properties.

Profeſſor Venturi. The only motion we obſerved was the re ſult of the molecular attraction, which determined a great number of the fragments diſperſed over the ſurface of the water to collect in one point.

Is ſoluble in alcohol.

It is wholly and readily diſſolved in alcohol, from which water ſeparates it unaltered.

Action of dilute nitric acid.

Nitric acid, at thirty degrees of *Beaumé*'s areometer, poured on this new camphor, had no action on it, even after ſeveral days of contact; although the ſame acid diſſolves the common camphor with the greateſt facility, becoming covered with an oily ſtratum, and water precipitates pure camphor from it.

Of concentrated nitric acid.

Very concentrated nitric acid had not, at firſt, any action on our camphor; but after a few ſeconds the ſolution is effected, and at the ſame time a diſengagement of nitrous gas is manifeſted: water rendered this ſolution very ſlightly turbid. The ſame acid, on the contrary, diſſolved the camphor of the ſhops rapidly, diffuſing white vapours. This ſolution took a beautiful fire-red colour, and water ſeparated pure camphor from it in flocks.

It is not diſſolved by acetic acid.

Neither did acetic acid, which perfectly diſſolves the common camphor, effect the ſolution of this. By heat, it ſoftened and ſeemed to diſſolve; but on cooling, it was wholly collected on the ſurface of the liquid, with all its properties.

We afterwards examined the liquor called the *mother-water of camphor.*

Phyſical properties of the mother-water. Peculiar oil.

This brown tranſparent liquor, lighter than water, heavier than alcohol, fuming by contact of the air, of a peculiar odour, ſwam above a thick reddiſh oil, the ſmall quantity of which obliged us to neglect the examination of it.* The taſte of this mother-water was ſharp; it did not mix with water, nor yield its acid to it. Agitated in this liquid, its particles remained diſperſed for a long time, before they collected on its ſurface.

Soluble in alcohol and ether.

Alcohol and ether diſſolved it completely; but the addition of water ſeparated them.

Inflammable.

Inflamed in a capſule, it burns without a reſidue, diffuſing a conſiderable quantity of very odorours ſmoke.

Action of ſulphuric acid,

Concentrated ſulphuric acid poured into this liquor, blackened it, and diſengaged from it ſome vapours, and a very ſtrong odour of gazeous muriatic acid.

* We only aſcertained its ſolubility in alcohol and ſulphuric ether, and its inſolubility in water.

A ſolution

A ſolution of cryſtallized carbonate of ſoda produced a ſlight efferveſcence in it, diſcoloured it, rendered it lighter, and formed muriate of ſoda. and of carbonate of ſoda.

This muriated oil, diſtilled to dryneſs in a ſuitable apparatus, yielded ſome muriatic gas, ſome drops of thick oil, ſimilar to that mentioned above, a very light and very combuſtible perfumed liquid, which comported itſelf with water, alcohol, ſulphuric acid, and ſoda, in the ſame manner as before diſtillation; there was only leſs muriatic acid in it. The reſidue was a black matter, glutinous like pitch, diſſolving imperfectly in alcohol, and yielding muriatic acid to water. Products by its diſtillation.

We afterwards made a mixture of equal parts of oil of turpentine and muriatic acid at 25°. We agitated it from time to time, and then left it for ſeveral days. The oil took a reddiſh brown colour, the acid alſo became reddiſh: both of them preſerved their tranſparence, and at the point of contact of theſe two liquids, was perceived a concrete ſtratum, having ſeveral of the properties of camphor, but its quantity was ſo ſmall that we were obliged to neglect it. Mutual action of oil of turpentine and muriatic acid produces a ſmall quantity of a concrete body.

A fact of this kind is to be met with in the tranſlation of the Chemical Recreations of *Model*, publiſhed in 1774, by M. *Parmentier*. Previous diſcovery of this fact.

This philoſopher expreſſes himſelf as follows, in page 400 of the firſt volume of this tranſlation, in the additions which he has made to it:

" A very curious and very intereſting obſervation to natural philoſophy and chemiſtry, is, that M. *Marges*, ſurgeon, in a work, the object of which is the examination and chemical analyſis of different medicaments *, ſays, that he obtained, from the digeſtion of a mixture of fuming marine acid and oil of turpentine, ſmall ſaline concretions, which at the end of a certain time, became more conſiderable, and took the figure of a cryſtalline ſalt, in form of a parallelopipedon, while the oil which ſwam above them was coloured red, and acquired a very thick conſiſtence, &c.

It is very probable that theſe cryſtals were the ſame as thoſe which were obtained in ſuch great abundance from the action of the ſame gazeous acid.

The ſame oil was ſaturated with oxigenated muriatic acid gas; Oxigenated muriatic acid gas and oil of turpentine.

* Paris, 1774. Second edition.

gas; it acquired a deep yellow, retained its tranſparence, became a little more conſiſtent, but did not produce any camphor.

No camphor.

Action of ſulphuric acid.

Concentrated ſulphuric acid acting too violently on oil of turpentine, the decompoſition of which it ſpeedily effects, we made a mixture of equal weights of this oil and ſulphuric acid marking only 40° of the areometer. The action was confined to the point of contact, and was analogous to that of liquid muriatic acid.

Slow diſtillation of oil of turpentine.

Eight ounces of oil of turpentine were kept for three days in a glaſs alembic, placed on a ſand-bath, the temperature of which was maintained at about forty degrees of *Reaumur's* ſcale. This time being elapſed, we found in the recipient nearly four ounces of a very white and very light volatile oil, and in the capital many ſmall cryſtallizations of true camphor. The oil remaining in the cucurbit was of an amber colour, and very much thickened.

Action of muriatic acid gas on the other volatile oils.

To ſatisfy ourſelves whether the other volatile oils would furniſh camphor by the action of the muriatic gas, we incorporated them with it in different proportions; but did not obtain any precipitation. They became very black, and contained a great quantity of heavy oil, &c.

CONCLUSION.

General reſults.

1ſt. That the production of camphor, announced by M. *Kind*, is uniform in the proportions he indicates, and of whatever ſoil the oil of turpentine may be.

2nd. That the camphoric product may be eſtimated at about half the quantity of the oil employed.

3rd. That a greater quantity of the gas does not add to the product, nor does it deſtroy it.

4th. That this camphor may be purified by water, by alkaline ſolutions, charcoal aſhes, quick-lime and alumine: and, that theſe three laſt ſubſtances are more eſpecially preferable.

5th. That it differs from what is extracted from the camphor-laurel, in its taſte, which is not ſo bitter, and in its odour, which is leſs penetrating; and, that it preſents an abſolute difference in its mode of action with the nitric and acetic acids, the firſt of which only diſſolves it by a reciprocal decompoſition.

6th.

6th. That, notwithſtanding theſe differences, it is not the leſs a beautiful new ſpecies of this immediate principle of vegetables to chemiſts; and, that it is to be wiſhed that phyſicians would try it, to aſcertain whether, as there is reaſon to believe, its action will be the ſame on the animal economy.

7th. That, during the operation from which this camphor reſults, there is formed a true combination of muriatic acid and volatile oil, having a reſemblance to what the ancients called *acid ſoaps*.

8th. That liquid muriatic acid, and dilute ſulphuric acid, not mixing with oil of turpentine, their action is confined to their point of contact.

9th. That the mode of action of the muriatic gas, in this circumſtance, may be explained by the elegant theory which MM. Fourcroy and Vauquelin have given of the action of ſulphuric acid on vegetable ſubſtances in general.

10th. That, notwithſtanding camphor is ſet free by the ſlow diſtillation of oil of turpentine, without addition, we are led to believe that ſo large a quantity of this matter could not exiſt ready formed in it, and that at leaſt a part is the reſult of the re-action of the principles of the oil, excited by the preſence of the acid gas, and its affinity for water.

11th. Finally, that the ſame proceſs is not applicable to the extraction of the camphor contained in the volatile oils of lavender and roſemary.

XVI.

Letter from JOSEPH HUDDART, *Eſq. F. R. S. on the apparent Enlargement of the Moon at low Altitudes.*

To Mr. NICHOLSON.

DEAR SIR,

IT was not until yeſterday that I received your Journals, Introduction. Nos. 36 and 37: In the former, reading a letter from C. L. occaſioned my referring to No. 35, page 164, of the Journal, for Mr. Walker's letter reſpecting the ſenſible magnitude of the

the horizontal moon above what it has in the zenith; and therefore preſent you with the idea I have entertained upon the ſubject.

Eſtimate of magnitude confined to the ſize of the image on the retina.

I am of opinion that the magnitude of any object can only be eſtimated (as to ſenſe) according to the ſpace or magnitude of its image upon the retina. The eye muſt (if this is admitted), therefore, be the ſole cauſe of the illuſion, for the moon's apparent magnitude increaſes as ſhe riſes towards the zenith.

The eye is formed not only to alter its aperture, but alſo its focal diſtance. Elongation of the eye will enlarge the image, though the angular magnitude remains unaltered.

The eye by the power of the muſcles can, at the will of the obſerver, not only be directed towards the object, in order to receive its image upon the uſual part of the retina, but acquire a proper convexity for diſtinct viſion, or focus of the refracted rays upon the retina; and alſo to contract or extend the area of the pupil according to the quantity of light. This admirable property may be obſerved by every one in paſſing out of a light room in the night, and alſo in the eyes of the brute creation; but I conceive, that while the area of the pupil is enlarged in order to receive a greater quantity of light, the eye is protruded, or the focal diſtance between the pupil and retina is increaſed, which increaſes the ſenſible magnitude of the object, as the image upon the retina conveys it to our ſenſes, when viewed by the naked eye. This I diſtinguiſh from apparent magnitude, which we uſe when actually meaſured by an inſtrument.

Variation of the pupil, or of the aperture of a lens, cannot alter the image.

The variation in the magnitude of the image upon the retina, certainly cannot ariſe from the variations in the dimenſions of the pupil, which is contrary to the law of dioptrics; for, on obſerving the ſun, I do not admit light through one-twentieth part of the area of the object glaſs, which is 3.8 inches in diameter, and yet there is not any difference in the apparent diameter; or, whether the rays are admitted at the center or any other part of the object-glaſs, the obſervations are equally good. But I conſider it as a natural conſequence, that when the pupil of the eye is enlarged, the focal diſtance is increaſed, and which conſtantly ariſes from a diminution of light, even without our attention; but thereby our ſight is aſſiſted in conſequence with more light and power, which is ſome compenſation.

The elongation of the eye is ſuppoſed to take place whenever the pupil is enlarged;

and as this takes place when the moon is leaſt lu-

The light from a celeſtial object near the horizon, is diminiſhed by paſſing a long diſtance through a groſs atmoſphere;

hence a ſenſible enlargement takes place in the moon as well as other bodies, which diminiſhes as the altitude increaſes, and the light leſs interrupted. minous, it then appears largeſt.

I am, Sir,

Your moſt obedient,

J. HUDDART.

Highbury Terrace, Jan. 28, 1805.

XVII.

A Memoir on Milk and the Lactic Acid. By Cit. BOUILLON LAGRANGE *.

SECTION I.

The preſent State of our Knowledge reſpecting the Lactic Acid.

SCHEELE and Deyeux, and Parmentier, are the chemiſts who have moſt contributed to our knowledge of the acid of milk. Hiſtory of experiments on milk, and the authors of diſcoveries, &c.

The experiments made before the time of theſe ſkilful philoſophers, are thoſe of Hoffman, Boerhaave, Homberg, Geoffroy, Rouelle the younger, and Baumé. But the analyſis of animal matters was then in ſuch an imperfect ſtate, that little can be gathered from their experiments.

Scheele directed his labours with very different views. His well conducted experiments led him to determine the characters and properties of an acid afforded by the ſerum of milk But not withſtanding the labours of this celebrated man, we have ſome ſubjects of inquiry left for examination. Scheele.

Beſides the great number of its ſaline compounds, as Fourcroy remarks, which are ſtill wanting to the ſcience, Lehale has Imperfect ſtate of our knowledge of the lactic acid.

* Annales de Chemie, No. 150. vol. 50.

The author in a note mentions his having learned on the evening when his Memoir was read, that Meſſrs. Vauquelin and Thenard had each been ſeparately employed on the ſame ſubject: and on that occaſion he then finds its neceſſary to ſay, that moſt of the experiments he deſcribes, are the reſult of obſervations made by ſeveral pharmacians, at the ſitting of the ſociety of pharmacy, the 15 Nivoſe, in the year 12. The verbal proceſs of that day, ſigned by Parmentier, preſident, and Delunel, ſecretary, of which an extract was ſent to the Philomathic Society, aſcertains the experiments he had then made, and the obſervations of of the ſeveral members of the ſociety of Pharmacy.

not

not pointed out the action of fire upon this acid, nor its ſpontaneous alteration in the air, the manner in which it comports itſelf with the nitric acid, &c. It is not known whether it be totally decompoſed by this laſt, or converted into ſome other acid, particularly the oxalic acid. We are entirely unacquainted with the nature of its compoſition: Though it preſents properties which reſemble thoſe of the acetous acid, and lead to the probability that it nearly reſembles it; we cannot yet rank it with the vegetable acids. On the other hand, nothing deciſive can be aſſerted with reſpect to its animal nature, becauſe no experiment has yet indicated the preſence of azote, and it is yet unknown, whether it may afford ammonia in its decompoſition; whether it be putreſcible, or convertible into pruſſic acid, &c.

This general ſtatement while it gives us a view of our knowledge of the lactic acid traces, as it were, the ſteps neceſſary to be purſued in determining the place which this acid ought to occupy, as to the number of well eſtabliſhed chemical facts.

SECTION II.

Concerning Milk and the caſeous Matter.

On milk, and its conſtituent parts.

Before I proceed to deſcribe the experiments I have made on this article, it will be uſeful to preſent ſome reflections on milk, and its conſtituent parts.

Guiton, in the Encyclopédie Methodique, offers two queſtions reſpecting the exiſtence of an acid in milk. He expreſſes himſelf thus:

Whether whey exiſts ready formed in milk.

Does the whey exiſt in milk ſuch as it is found after the ſeparation of the other conſtituent parts? Does it manifeſt acid properties on any other account, than becauſe it holds ſalts in ſolution, as all the analyſes ſuppoſe?

Milk cannot be reproduced by the mixture of its parts.

If whey, adds the ſame chemiſt, exiſted in milk, in the ſtate it exhibits after the ſeparation of the butter and cheeſe, we ſhould be able to reproduce milk by mixing theſe three principles again in the ſame proportions. But as this is not the caſe, he concludes, that whey is the product of a true fermentation.

The examination of this firſt queſtion founded on experiment, ought, I think, to prove that it will not be ſufficient to ſhew whether whey exiſts in milk, that we ſhould mix the three principles in the ſame proportions, and re-produce that fluid; firſt, becauſe theſe principles are no longer the ſame; and

and ſecondly, becauſe whey formed by the uſual proceſs, is more or leſs acid.

Experiment 1. Recent milk gives a red colour to the paper and the tincture of turnſole. Exp. 1. Milk reddens turnſole.

Experiment 2. If milk be diſtilled in cloſe veſſels, and the product ſeparately taken, the firſt is not acid; the ſecond reddens turnſole, and ſlightly precipitates the nitrate of ſilver; and the third has no action on the tincture. At this period, the milk is not yet decompoſed, but ſtill reddens turnſole. It appears that the remaining acid is no longer volatile, but is retained either by the animal matter, or by ſome other ſubſtance. Exp. 2. It affords acid by diſtillation, and continues acid.

Experiment 3. If milk be coagulated with a mineral or vegetable acid, the whey is ſeparated, without retaining a particle of the acid made uſe of. Exp. 3. Whey of milk coagulated by an acid, retains none of the acid itſelf.

I muſt however obſerve, that ſome deception would follow, if the ſerum only were examined, which is obtained by the ſulphuric acid, or by allum, becauſe the precipitate afforded by barytes, is not entirely re-diſſolved by the nitric acid. I am ſatisfied that this ariſes only from a ſmall quantity of ſulphurate of potaſh contained in the ſerum, as is evidently proved, when the whey is ſeparated by any other ſubſtance not of an acid nature.

It is not true therefore as many chemiſts have advanced, that the acid ſeparates the caſeous matter, by uniting with the ſerum. Exp. 4. Whey from milk ſpontaneouſly decompoſed, is alſo acid.

Experiment 4. If milk be expoſed to the air at the temperature between 12 and 20 degrees (about 66° Fahrenheit) the ſeparation takes place in twenty-four hours. The curd has a more acid flavour than that of experiment 3. Hot water cauſes it to loſe its ſour taſte, and acquires the property of reddening turnſole.

We here find nearly the ſame properties in the cheeſy matter, when ſpontaneouſly formed, and when ſeparated by acids.

Experiment 5. The ſame experiment being made with the pneumatic apparatus, was attended with no abſorption of air, or diſengagement of elaſtic fluid. The only difference was, that the ſeparation of the cheeſy matter did not take place for ſeveral days. Part floated on the liquid, and the other occupied the bottom of the veſſel. Exp. 5. Milk ſpontaneouſly decompoſed, neither abſorbs nor gives out air.

Experiment 6. A bottle was filled with freſh milk, and well corked. Some days afterwards, the cheeſy matter had ſeperated. As ſoon as the ſeparation was complete, the cork was intended to be extracted, but it flew out with violence and noiſe, Exp. 6. Milk decompoſed ſpont. in a cloſed bottle, gives carbonic acid.

noiſe, indicating the preſence of an elaſtic fluid. Upon ſhaking the bottle, a greater quantity of gas was diſengaged, which being collected and examined, had the ſame characters as carbonic acid.

The fluid as well as the curd had a ſharp acid taſte, which became leſs pungent, as the carbonic acid was diſengaged by agitation.

The fluid, after having been heated, did not appear to be more acid than ſerum, obtained by expoſing milk for the ſame time to the air.

The ſame experiment was made with milk which had been boiled about half an hour, and the reſults were the ſame.

Exp. 7. Qualities of the cheeſy matters.

Experiment 7. The cheeſy matters of the third, fourth, and fifth experiments, differ eſſentially in taſte and conſiſtence.

That of No. 3. is dry and firm; that of No. 4. is more divided, leſs dry, and partly ſoluble in water; and laſtly, that obtained in the experiments No. 5 and 6. is not clotted, but is more light, and does not unite into a maſs, till after ſeveral hours.

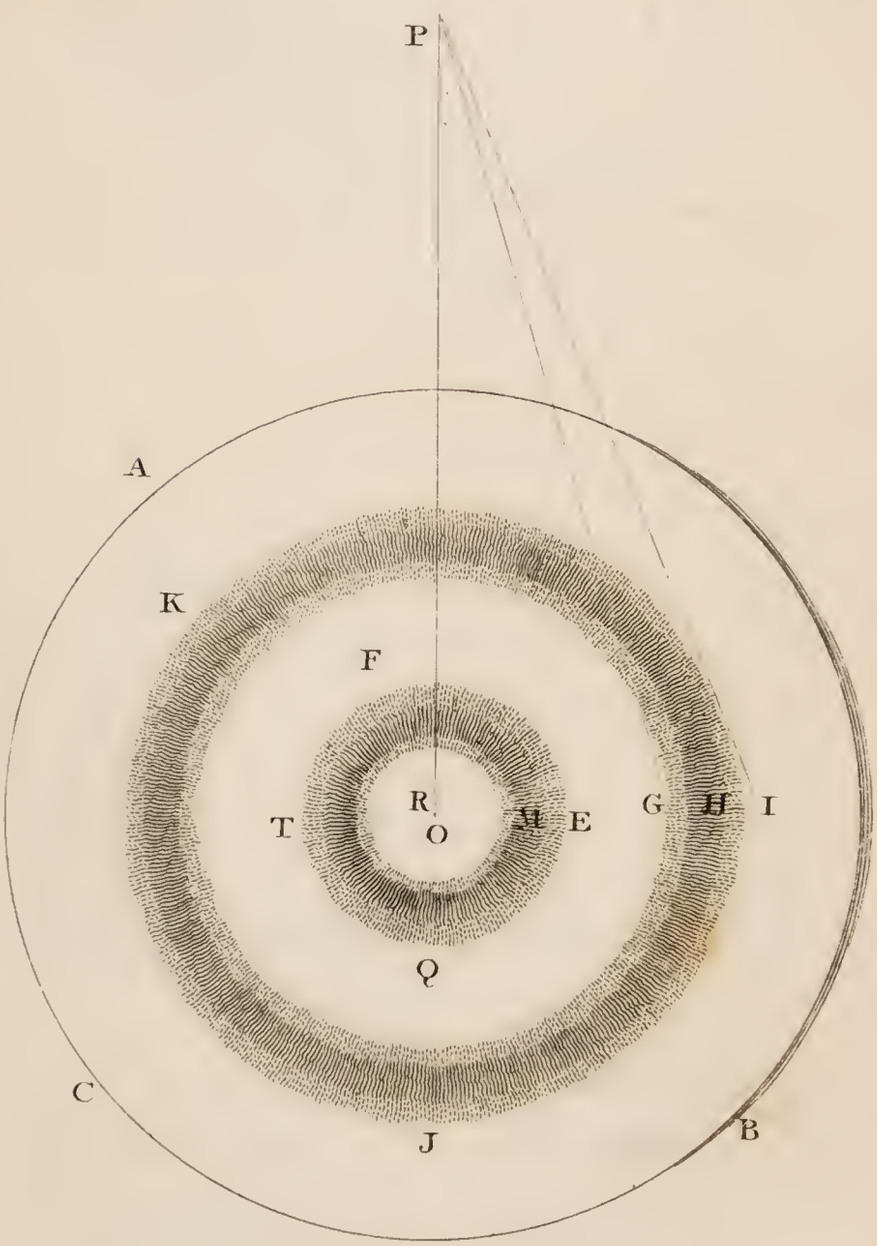
The cheeſy matter, therefore, requires particular properties, according to the ſubſtances and the proceſſes employed to ſeparate the ſerum.

Deductions from the facts.

Theſe firſt experiments lead us to the following reflections, that, 1. Milk does not require to be decompoſed, in order to manifeſt the preſence of an acid. 2. That this acid is mixed with ſalts, ſugar, and animal matter. 3. That the acid in milk is diſengaged, though it is not very perceptible, but by re-agents. 4. That the contact of the air is not neceſſary for ſeparating the conſtituent parts of milk. 5. That the coagulating ſubſtances merely facilitate the ſeparation of the cheeſe, either by forming a new compound, or by more immediately condenſing the particles together, when the cheeſy matter requires new properties: or laſtly, by expoſing the milk to the air. By degrees, a part of the ſugar of milk is decompoſed; carbonic acid is formed, of which one part is diſengaged, and the other facilitates the ſeparation of the curd; caloric alſo favours the attractions of coagulating matters for the curd. They all act differently, for alcohol, which alſo poſſeſſes this property, affords other reſults; the curd being to a certain point ſoluble in water. We cannot therefore as chemiſts have aſcribed theſe effects to the ſolution of thoſe ſubſtances in water, and their greater attraction for the liquid than is exerted by the cheeſy matter itſelf.

(To be continued.)

Mr. Gough's theory of the Augmentation of Sound by communicated vibrations.



Mutlow Sc. Rupert Ct.

Capt. Mortlock's Method of surveying, by very simple Instruments.

Fig. 1.

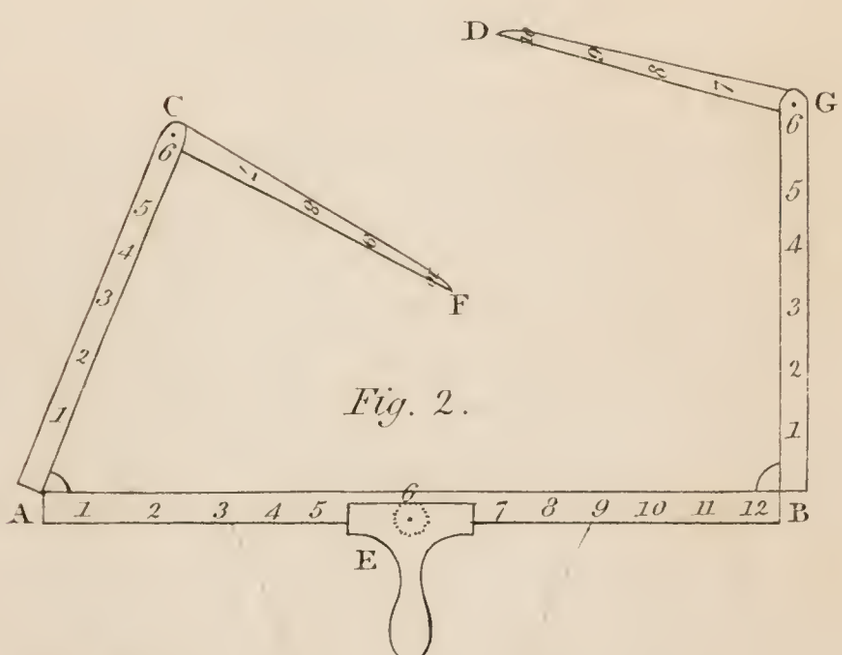


Fig. 2.

Instrument for taking Designs from Nature.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MARCH, 1805.

ARTICLE I.

Hiſtorical and Critical Obſervations relating chiefly to the Invention of the Teleſcope. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

ALTHOUGH I was aware that the experiment on the incompreſſibility of water was to be found in Lord Bacon's work, yet I was happy to ſee that Sir H. Engleſield had taken the trouble of reminding your readers of the original inventor of it. I am always glad when I ſee any thing which may recall our attention to the works of our illuſtrious countryman. They abound with inſtruction, and, in many inſtances, his conjectures may be compared to the Queries of Sir I. Newton, which contain the foundations of the greateſt diſcoveries. Incompreſſibility of water.

In the ſame paper Sir H. Engleſield has given us a very curious paſſage from Kepler; and although I cannot perſuade myſelf that he is accurate in his interpretation of it; yet I think the argument has been by no means accurately ſtated by Aletes *. I ſhall therefore beg your permiſſion to inſert ſome additional remarks on a ſubject, which certainly poſſeſſes a conſiderable ſhare of intereſt. I muſt beg leave, likewiſe, Remarks on the communications of Sir H. C. Engleſield and Aletes.

* Vol. X. p. 92.

Obſervations relating to the invention of the teleſcope.

previouſly to remark that Sir H. Englefield has not aſſerted that Kepler was the original inventor of the teleſcope. The only inference which can be juſtly drawn from his words, is that they were invented as early as the year 1607. No one, indeed, who had attentively read Kepler's dedication of his Dioptrice, could have entertained ſuch an opinion. He preciſely ſtates * that ſince "ad magnum cumulum inventionum hujus ultimi ſæculi acceſſiſſet Arundo Dioptrica, nequaquam inter vulgares commemoranda machinationes, circaque eam, alii de palma primæ inventionis certarent, alii de perfectione inſtrumenti ſeſe jactarent, ... Galilæus vero ſuper uſu patefacto in perquirendis arcanis Aſtronomicis ſpecioſiſſimum triumphum ageret; . . . . Ego ductus honeſta quadam æmulatione novum mathematicis campum aperui . . . . . cauſarum lege geometrica demonſtrandarum quibus . . . effectus inniterentur."

Before the invention of teleſcopes, aſtronomical obſervations were ſometimes made with tubes, but more generally with rods (regulæ) which had ſights fixed at each end. Theſe inſtruments were generally called Dioptra, and the apertures in the ſights were called foramina dioptrorum, not perſpicilla. Ptolemy in his Almegiſt † deſcribes the inſtrument, which is generally known by the name of his Triquetrum. It had ſights at each end, with apertures in them, which Georgius Trapezuntius, the tranſlator, calls foramina: he mentions, indeed, no particular name for this part of the inſtrument, but Theon in his commentary ‡ deſcribes the conſtruction of it to be ſuch, that the whole moon would appear through the ſights εν τη διοπτρικα. Proclus Diadochus, in his Hypotypoſis Aſtromicarum Poſitionum,** mentions the Dioptra of Hipparchus, which was an inſtrument of the ſame kind: ſee Riccioli Almegiſtum Novum, vol. I. B. 3. C. 10. § 4. But we can have no further doubt upon the ſubject, if we turn to Flamſtead's Hiſtoria Cæleſtis, vol. III. p. 97. He there ſays that "haud pauci

* Pp. 53, 54. Lond. 1653. † Book 5, chap. 12.

‡ P. 258. Baſil, 1538. Notwithſtanding what Hardouinus ſays, I have no doubt, but that the Dioptra mentioned by Pliny in his Natural Hiſtory, B. 2 C. 69. was an inſtrument of this kind; but no deſcription of it is given, and therefore I cannot argue upon it in this place.

** P. 399. Baſil, 1541.

obſervationum

obſervationum Tychonicarum errores ex nudorum Dioptrorum uſu neceſſarie conſequuntur." Now it is only neceſſary to examine the Aſtronomiæ Inſtauratæ Mechanica, and we ſhall ſee that almoſt all the inſtruments, which Tycho Brahe uſed, are fitted with ſights, which were ſome perforated with ſmall holes, and ſome divided by narrow ſlits: this part of the inſtrument he called *dioptra*, the perforations he called *foramina*, and the ſlits *rimulæ*. Obſervations relating to the invention of the teleſcope.

I could quote further proof that theſe words were commonly uſed in the ſenſes which I have affixed to them, but it would be not only unneceſſary but tedious. It muſt, therefore, remain with Aletes to produce as good authorities for the alledged difference between *perſpicillum* and *perſpicillum vitreum*. I cannot help thinking, however, that the very title-page of Kepler's Dioptrice will be fatal to his explanation; for there we find that mention is made of the diſcoveries of Galilæo, made "*ope perſpicilli*" after the publication of the Siderius Nuntius. Here the word *perſpicillum* is undoubtedly uſed for the teleſcope itſelf, and before I conclude what I have to ſay, I ſhall quote a paſſage from Galilæo, which is ſtill more contradictory to the ideas of Aletes.

Although I differ from your correſpondent with reſpect to the arguments, which he has ſtated to you, ſtill I agree with him in his general concluſion, that Kepler did not mean a teleſcope by the word *perſpicilla*. We learn, indeed, from Borelli's book de vero inventore teleſcopii, that * one account makes the invention as early as the year 1590; but when we compare this with the depoſition of Sara Gædarda † and what is ſtated by Galilæo in (p. 10 of) his Sidereus Nuntius, it ſeems moſt probable that teleſcopes were not known before the year 1609. If Zachary Janſens was acquainted with them before that period, he ſeems not to have publiſhed his diſcovery to the world. Now Kepler, in the place above quoted from the dedication of his Dioptrice, takes an opportunity of praiſing the invention in the higheſt terms, and as he did this, when it was public, it is probable that he would have done ſo ſtill more warmly, if he had been the firſt who publiſhed an account of obſervations made with them; but there is nothing of this kind in the paſſage alluded to in his book de cometis.

* P. 25. † p. 31. This makes the invention about 1611 or 1613, but "de certo præfixo tempore non potuit dicere."

Obſervations relating to the invention of the teleſcope.

He was looking at the ſtars with a friend on the bridge at Prague, and they obſerved one in particular. "Vidi" * he ſays, "ſtellam ſub urſa, majorem cæteris per perſpicilla intuitus, quæ æquale cæteris fixis lumen mihi ſine perſpicillis diffundere videbatur." It is not likely that the perſon, who mentioned the firſt uſe of ſuch an inſtrument as a teleſcope, would confine himſelf to ſo dry an account of the effects produced by it. But there is another argument, which may be drawn from the book itſelf, and that is, when Kepler is giving an account † of his obſerving a comet in the year 1618, he uſes the word teleſcopium and not perſpicillum. "6 Sept. jam nulla amplius cunda viſu nudo potuit obſervari et teleſcopio inſpectus ſatis magnus apparuit." "Sept. 23. Planitudo major ſine teleſcopio, quam per illud." ‡

But what puts it beyond doubt that Kepler was not in poſſeſſion of a teleſcope ſo early as the year 1607, is that we hear of no great diſcoveries which he made about that time by obſerving the heavens. Monſieur de Moutucla, indeed, ſays § that "Uniquement appliqué a determiner avec preciſion les mouvemens celeſtes, cet homme celebre faiſoit peu d'uſage du teleſcope." He probably thought himſelf juſtified in this, when others were employed in the more mechanical labour of obſervation. When he had no ſuch fellow-labourers, it is impoſſible that a man like Kepler could have neglected an opportunity of obſerving, or have concealed the means of doing ſo; and if he had not done both, we muſt have had ſome notices remaining either in his own writings or thoſe of others.—— We muſt recollect likewiſe, that he was the firſt who underſtood the theory of teleſcopes and the combination of lenſes, and we muſt not forget, that theſe diſcoveries being once made, the greateſt difficulty was overcome; it was only neceſſary to uſe the teleſcope and mark the objects which it preſented to the ſight.

From what I have ſtated there ſeems to be conſiderable difficulties attending either of the ways, which have been propoſed for interpreting the word perſpicilla. But there are ſome paſſages in the Sidereus Nuntius, which ſeem to aſſiſt us in affixing a meaning to the word, leſs exceptionable than the one hazarded by Aletes.

* $\frac{16}{26}$ Sept. 1607. † P. 48. ‡ P. 58.

§ Hiſtorie des Mathematiques, vol. II. p. 234.

In

In p. 11. Galilæo ſays "tubum primo plumbeum mihi paravi, in cujus extremitates vitrea duo perſpicilla . . . aptavi." Again in p. 12. he ſays "dum nulla in tubo adeſſent perſpicilla," the rays paſs in one direction, but "appoſitis perſpicillis," they will paſs in another. Hence it appears that Galilæo makes no difference between *perſpicilla* and *vitrea perſpicilla*, but uſes both indiſcriminately for the glaſſes of his teleſcopes. I am inclined indeed to believe that the original meaning of *perſpicillum* was like that of *conſpicilium* a mere lens; that when lenſes were combined in teleſcopes it was uſed both for the glaſs and the inſtrument, until the word *teleſcopium* became common. I am not, however, prepared to bring forward ſufficient authorities for this account of the word, and I have not leiſure at preſent to ſearch for them. It is ſufficient for my purpoſe that we are juſtified in underſtanding the word as meaning a mere lens; for if we conſider it as ſuch in the paſſage under conſideration all the difficulty vaniſhes.—— Kepler complains in this very book *de cometis* * of the weakneſs of his ſight; he might, therefore, have probably uſed a ſimple lens to aſſiſt it. Monſieur Dutens † has clearly proved that the convex lens was known to the ancients, and at the ſame time ‡ that Borelli calls Zachary Janſens, "conſpiciliorum artifex peritiſſimus," he ſpeaks of his concave as well as convex glaſſes. Obſervations relating to the invention of the teleſcope.

There is one circumſtance attending the obſervation itſelf, which ſeems to give ſome ſupport to my interpretation, which is, that it was not made at a time when Kepler was carefully examining the heavens for any new phenomena; but in a walk which he accidentally took with his friend. Now he may eaſily be ſuppoſed to have carried about with him a ſimple lens to correct any fault of his eyes, but it is ſcarcely probable that he would have had with him a teleſcope, made, poſſibly like Galilæo's, of a long leaden pipe. I ſcarcely need add, that whatever clears and ſtrengthens the ſight will diminiſh the apparent diameter of a fixed ſtar, and conſequently increaſe the relative magnitude and light of the comet.

* P. 30. Inſtrumenta Tychonica, &c. &c. et oculos vegetos requirunt, quæ omnia mihi defuerunt, 1607, and in p. 48. Debili ſum viſu, 1618.

† Decouvertes des anciens. Partie 3, chap. 10. §. 278. ‡ P. 25.

I am

Obſervations relating to the invention of the teleſcope.

I am conſcious, Sir, that what I now ſend you, is put together in a very haſty manner. Were my time at my own diſpoſal, I would endeavour to make it more fit to meet the public eye; but I am induced to ſend you theſe hints in their preſent ſtate, becauſe I rely as much upon the candour as on the learning and abilities of Sir H. Englefield. If I am miſtaken, he will ſet me right; and if I have any foundation for my opinion, he can eaſily illuſtrate my hypotheſis by the paſſages which agree with it, in the old writers on optics and aſtronomy. I am, Sir,

Your obliged humble ſervant,

E. O.

Oxford, Feb. 10, 1805.

P. S. Since I finiſhed the letter, which I ſent to you on the 10th inſt. I have met with a paſſage, which appears to me to decide the queſtion on which I wrote to you. It is in Kepler's book, which he publiſhed at Francfort in 1604, under the title of *Aſtronomiæ pars optica.* The beginning of it is entitled *Paralipomena in Vitellionem.* He diſcuſſes the nature of the eye, and at p. 178. he begins a ſet of propoſitions on the manner in which it acts as the organ of viſion. The 28th of theſe propoſitions is that which makes moſt immediately for my preſent purpoſe; it is at p. 200, and he enumerates it in the following words.

"Qui remota diſtinctè vident, propinqua confuſè; iis per-
"ſpicilla convexa proſunt. Qui vero confuſè vident remota,
"juvantur concavis perſpicillis."

The mere quotation of theſe words is ſufficient for the proof of what I have aſſerted; but as it may afford ſome amuſement to your readers, I will add his remarks upon them as ſhortly as poſſible.—"Quanta admiratio" he ſays, "rei tantæ tam late propogatum uſum: & tamen cauſum ignorari hactenus." J. B. Porta, he adds, profeſſes to give the reaſon in his optics: but this book he was never able to meet with. Kepler then very ably refutes the opinion of thoſe, who ſuppoſe this effect to be produced by the increaſing or diminiſhing of the apparent magnitude of the object. He offers with conſiderable diffidence his own method of accounting for the phenomenon. He ſuppoſes that in the caſes mentioned in the propoſition the viſion is imperfect, in as much as without the intervention

tervention of convex or concave lenſes, the apices of the different cones of rays would fall either before or behind the retina. He ſupports his theory by experiments which he tried on ſhort and long-ſighted perſons; and quotes the paſſage from Ariſtotle, in which he points out the difference between the myops and the preſbytes.

Feb. 12.

II.

Conſtruction of a Line in a Circle, nearly equal to the Side of a Square of the ſame Superficies as that of the Circle itſelf. With Remarks on Pendulums and other Objects. By Mr. J. Whitley Boswell.

To Mr. NICHOLSON.

SIR, *Feb.* 9, 1805.

Introduction.

HAVING often found a difficulty in computing the preſſure of fluids in tubes and cylindrical veſſels, from all the calculations relative to the gravities of different bodies, which I was acquainted with, being adapted for cubic meaſure, it appeared a very deſirable matter to find an eaſy method of converting round into ſquare meaſure, which induced me at different times to attempt it, and as often to lay it aſide, from not being able to arrive at any ſatisfactory reſult. But lately having had occaſion to reſume this ſubject, I have diſcovered a method of coming ſo very near the truth, that if it ſhould turn out to be in reality not ſo exact as it ſeems to me, it will ſerve ſo well for common computations, and promiſes on that account to be of ſuch utility, that I am induced to ſend it to you for publication, if you approve of it.

To convert round meaſure into ſquare.

As this ſubject has been by many looked on as ſomewhat chimerical, it may be therefore not amiſs to mention, for thoſe of your readers who are not very converſant in geometrical ſtudies, that a mechanical method of computing ſquare meaſure from round is a very ancient and well-known problem; but this method being difficult to apply to the inſide of tubes, and not capable of being uſed in diagrams or drawings, has made another method deſirable: mine for this purpoſe is as follows:—

To

To find a right Line, the Square of which ſhall be equal to a given Circle.

Problem. To find by conſtruction the ſide of a ſquare equal to a circle.

Draw two diameters (I F and C T) biſecting each other at right angles in the center (O) of the given circle; biſect one of the radii ſo formed (O T) in W, and from the extremity (I) of the next adjoining radius, through W draw the right line (I B) to the circumference: This line (I B) is the line required.

Proof.

Proof by trial.

Let a ſquare be formed equal to the given circle by the third corol. prop. fifth, of Archimedes; then take a ſquare formed by the line I B, and place it on this other ſquare, ſo that one angle and the ſide adjacent to it of one, ſhall fall on one angle and the ſide adjacent to that angle of the other; then will it be ſeen that all the other angles and ſides of each will coincide, and the whole of one be equal to the whole of the other.

This kind of proof is nearly the ſame as that of the fourth prop. firſt book of Euclid, on which ſo many other propoſitions depend; and having often tried this method in the above manner, I could never perceive any difference between the two ſquares: In thoſe trials I uſed circles of card paper for the more exact meaſurement of the circumference in the mechanical proceſs directed in the method of Archimedes.*

To the above I have to add the farther proof which follows of the exactneſs of my method, which may make it appear ſtill more certain.

Sometime after I diſcovered the above, looking into a work of the learned Kircher for a ready method of deſcribing a pa-

* In order to ſhew how near Mr. B.'s conſtruction approaches to the truth, we may obſerve, that when the diameter is = 1, the area is = 0.7854, and the ſide of the equal ſquare = 0.8862. But in the figure, I W is found by adding the ſquare of the radius to that of the half radius, and extracting the ſquare root; and then by the property of ſimilar triangles, as the radius is to I W, ſo is the diameter to I B, which will be 0.8944 when the diameter is = 1. But this line exceeds 0.8862, or the true ſide of the ſquare, by 0.0082, or nearly one hundredth part. N.

rabola which I knew was inſerted in it, I perceived therein alſo a geometrical method of finding a right line equal to any given portion of the circumference of a circle; and having by this method drawn a right line equal to the fourth of the circumference of a circle, and with it and the diameter formed a parallelogram equal to the circle, and then a ſquare equal to this parallelogram, in the uſual manner: I afterwards formed another ſquare equal to the ſame circle, according to my method before deſcribed, and found no perceivable difference between it and the former ſquare.

The diſcovery of a fact in geometry often leads to another; one of this kind I have to add here, which is, that a right line (B E) drawn from the extremity B of the line I B, at right angles through the oppoſite diameter (I F) to the circumference, will be equal to a fourth of the circumference.

The problem of Kircher above mentioned, may be found in the *Ars magna Lucis et Umbræ*, prop. 3, lib. 3. pars 2, page 239, and the two following pages.

As the circles which I uſed were under three inches diameter, it is poſſible on ſo ſmall a ſcale a minute error might not be viſible; but even in this caſe it muſt be very inconſiderable.

Properties of pendulums, &c.

I take the opportunity of forwarding the above to ſend an explanation in reply to the annotations on my paper on pendulums, which I hope you will do me the favour to inſert, as you have in ſome degree called on me for it.

I beg leave to obſerve, that in that paper I have endeavoured to put a marked diſtinction (though perhaps not ſufficiently forcible) between the obſervations which admitted of proof, and ſpeculations on new ſubjects: The latter may be conſidered to commence with Huygens' pendulum; and the others on ſimilar principles, along with this, were merely deſigned as objects *propoſed for experiment*, not as things proved, and nothing is inſerted about them but what was intended to be noted as conjectural by the mode of expreſſion.

I conſider it uſeful to propoſe experiments (excluſive of my partiality to this mode of aſcertaining facts), becauſe among your numerous readers there might be ſome who, with little trouble or expence, have opportunities to make trials of this kind,

Properties of pendulums, &c.

kind, from the nature of their employments furniſhing them with all the materials neceſſary ready at hand, and who have ſufficient intereſt in the ſubject to induce them to undertake them.

I acknowledge that (as you obſerve) I did not ſtate all my reaſons for thinking that Huygens' circular pendulum, and the others propoſed on the ſame priſciples, would not be ſo liable to alter their notation of time, as thoſe in common uſe, from a change of temperature; but this was cauſed by the other ſubjects contained in that paper having extended it to ſuch a length as to oblige me to curtail this and other matters, and leave out part of what I had written relative to pyrometers alſo. My reaſon for thinking well of Huygens' pendulum proceeded partly from my reſpect for his opinion, founded on the high idea I have of his mathematical erudition; and becauſe it appeared to me, that the ſize of the circle produced by the gyration of the ball, depending on the impulſe of the moving power more than on the length of the ſuſpending ſtring; that if the ſtring lengthened a little, the circle would not be thereby increaſed; and even if it was increaſed, that the effect of the paraboloidal lamina would prevent its altering the time of each revolution of the ball (granting the theorem of Huygens, which I inſerted, to be true); for though I was aware that the parabola generated would, by the lengthening of the ſtring, become of ſomewhat leſs curvature, yet I thought this change would be ſo minute as not ſenſibly to affect the time, eſpecially as a change apparently greater had not affected it in a ſimilar caſe, which will be mentioned a little farther on, but of this I had doubts then, as appears from what is remarked in page 77, where I mention that I thought the other conſtructions which I propoſed might be better on account of *the ſtring of that of Huygens being liable to lengthen.*

I recommended the other pendulums, on ſimilar principles, for trial, becauſe though the expanſion will increaſe the dimenſions of the parabolical and cycloidal curves, yet ſtill they will not ceaſe to be thoſe curves, which Huygens has proved to have ſuch remarkable properties for the regulation of pendulums; and as it has been found by experience, that a pendulum vibrating in a ſmall arc of a large circle, has the ſame accuracy as when moving in a cycloid, I imagined that there could not at leaſt be more difference between the effects of

two

two cycloids or parabolas a little differing in ſize, than between the cycloid and large circle; and beſides, as the cycloidal cheecks uſed by Huygens (which he found to ſucceed ſo well) were liable to the ſame alteration by expanſion, I looked on his numerous experiments as confirmations of this opinion. Properties of pendulums, &c.

In reply to the obſervation relative to the ſuperior effect of pendulums detached as much as poſſible, I beg leave to obſerve, that a movement interrupted or intermitting (ſuch as more or leſs takes place in clocks with oſcillating pendulums), is ſo very different from one which continues equally without any interruption (as that of the circular pendulum), that it does not ſeem to me concluſive to argue from the effects of one to thoſe of the other.

In the note relative to the circular veſſel encloſing the mercurial tube, I am inclined to imagine you were not aware that this veſſel was directed to be faſtened to the ſpindle ſo as to revolve with it, by which means there would be no lateral motion given to the air but by the friction of the outſide of the caſe; for which reaſon I mentioned that it ſhould be made very ſmooth externally: I alſo directed it to be covered, to prevent the effect of the current of air, which would otherwiſe paſs through it, cauſed by the centrifugal impulſe of the circular motion.

As to what I obſerved about the ſuſpending ſpring, I perhaps expreſſed myſelf too generally: I ſtill think, however, I could point out ſeveral inſtances of gridiron pendulums, where the compenſations were made of one bar againſt another only, without allowing any compenſation for this ſpring.

I think it neceſſary to mention among the emendations of my former paper, that the cycloidal bed of the rolling pendulum, propoſed in it, ſhould be formed of the ſame thickneſs in every part, as it might expand unequally if made thicker in one part than another: As a trial of this ſort of pendulum may be made with a common clock at a ſmall expence, I hope it will induce ſome one better ſkilled in ſuch experiments than I am, to make it, even if the others ſhould not be tried: and as a farther reaſon for expectation of ſucceſs in the trial, it ſhould be conſidered (in addition to what has been advanced in ſupport of the opinion, that the effect of the cycloidal bed would be but little altered by its expanſion), that,

Properties of pendulums, &c.

that, on account of the ſhortneſs of this bed, it could not poſſibly expand as much in length as the 39 inches of wire contained in a common pendulum-rod; and that, as the greateſt expanſion of the wire is certainly in the direction the moſt injurious to the effect of the pendulum, which is not the caſe with that of the cycloidal bed, it is probable that the rolling pendulum would be found ſuperior to that in common uſe.

With regard to the obſervations on pyrometers, I own I never ſaw that of Deluc; but as your deſcription of it is not ſufficiently minute to enable me to perceive how the ſtandard in it for meaſuring the different bars, could eſcape expanſion in an increaſe of temperature of the atmoſphere, I ſtill can conceive no method by which the ſtandard for meaſurement in any pyrometer, could be prevented from altering its length from the above cauſe, but by keeping it artificially at a fixed temperature, by ſome means diſtinct from the apparatus, ſimilar to thoſe of Mr. Ramſden, which you have mentioned. What I wrote on this ſubject was intended as a caution in future experiments, and if it has produced the more extended publication of means to avoid the error I pointed out, or ſhall hereafter do ſo, my view will be fully anſwered.

Your very humble ſervant,

J. WHITLEY BOSWELL.

III.

Some Remarks upon the Experiments by which Mr. Ez. Walker *has endeavoured to explain the apparent Enlargement of the Moon near the Horizon: with a Statement of ſome Facts upon which that Phenomenon ſeems chiefly to depend. In a Letter from* C. H.

To Mr. NICHOLSON.

SIR,

Mr. Walker's experiments adverted to.

I SHOULD, with your permiſſion, be glad to point out to your correſpondent, Mr. Ezekiel Walker, a theorem in optics, to which, it appears to me, that he has not paid ſufficient attention in the courſe of his ſtudies.

Rays

Optical theorem. The focus of a lens is ſhorter the larger the aperture, and the image of courſe ſmaller.

Rays from a diſtant object which paſs through a convex lens near its center, will have their focus more diſtant from the lens, than thoſe rays which paſs through it nearer to its circumference; and, conſequently, the image formed by the central rays, will be as much larger than that formed by the rays of the circumference, as the focal diſtance of the former ſhall exceed that of the latter. With a given aperture, the focus will be that point in which the greateſt number of rays coincide, for, *there*, will the image appear moſt diſtinct; and, conſequently, as we enlarge the aperture of the lens, the central rays muſt be combined with others more convergent, the new focus will approach the lens, and the image be, of courſe, proportionably diminiſhed.

This is contrary to Mr. Walker's reſult; in which he was probably deceived by the differences in brightneſs.

The total diſagreement of this *theorem* with the reſult of Mr. W.'s experiments, I can only account for by ſuppoſing this gentleman, while he enlarged the aperture of his lens, not to have paid attention to the increaſing brightneſs of the image, which, by making a ſtronger impreſſion on the retina, would appear to increaſe in magnitude with every enlargement of the aperture. If I might be allowed to offer my advice to Mr. W. I ſhould recommend him to repeat his experiments, and ſubſtitute, for the flame of the candle, a ſkreen having a circular opening covered with thin paper. This, with a light behind it, may repreſent the horizontal moon. Some characters ſhould be marked upon it, in order to determine, with more facility, when the image is moſt diſtinct. The precaution of reducing the different images, as nearly as poſſible, to the ſame degree of brightneſs previous to meaſurement, muſt not be neglected. I am ſurprized indeed, that the ſtructure of the eye, which Mr. W. profeſſes to have kept in view in the courſe of theſe experiments, did not point out to him the importance of this circumſtance, which is ſo admirably anſwered, in the natural organ, by the contraction and expanſion of the pupil.

Propoſal that he ſhould vary and repeat the experiment.

Phenomenon of the horizontal moon occaſioned in part by the ſuppoſition of greater diſtance, by the aerial perſpective, and other circumſtances;

As the phenomenon of the horizontal moon has of late much engaged the attention of ſeveral of your correſpondents, I will avail myſelf of the preſent opportunity to add a few remarks on that ſubject. It appears, at firſt view, that the moſt obvious method of accounting for the phenomenon, is the following: We believe the moon more diſtant from us when in the horizon than when in the meridian; but we ſee it,

it, in both caſes, under the ſame apparent angle, and, according to our uſual habits of judging of the magnitudes of objects, neceſſarily refer the idea of greater magnitude to the greater diſtance. The aerial perſpective, and various other circumſtances, which combine to make the horizontal diſtance appear the greateſt, have been ſo well and ſo often deſcribed, that it would be ſuperfluous to repeat them here. I muſt obſerve, however, that this appearance of greater diſtance in the horizontal moon, though the firſt and moſt obvious circumſtance which preſents itſelf to explain the illuſion, is not, in my opinion, that which has the greateſt ſhare in producing it.

but theſe have not the greateſt ſhare in producing it.

After having conſidered the ſubject with much attention, I am of opinion that the chief ſtreſs ought to be laid on the following particulars:

New explanation. The moon is rarely ſeen in the horizon.

We may be ſaid rarely to ſee the moon in the horizon. Our habitual acquaintance with it is at ſome conſiderable altitude. Hence it follows, that all the objects with which it comes in apparent contact, and with which we occaſionally compare its diſk, are near to us, and ſubtend angles proportionably large. Thus, for inſtance, we often ſee the moon loſe a large portion of her face behind the branch of a tree or a weather-cock, and totally diſappear behind a chimney. The clouds, too, which fleet before her, appear, from their proximity to us, on a gigantic ſcale. Theſe are the circumſtances under which we have inſenſibly formed our general ideas of the moon's magnitude. Let us now ſurvey it in the horizon. The caſe is widely altered. We have now an opportunity of comparing it with various large objects, which perhaps, being themſelves on the verge of a diſtant horizon, will be reduced to very trifling dimenſions; and thus the caſtle and maſſy cathedral will hide but a ſmall part of that globe, which we have commonly ſeen half eclipſed by a ſpout or a weathercock. The clouds too, reduced by diſtance, appear on a much ſmaller ſcale when compared with the moon. In ſhort, it is no longer the ſame moon we were acquainted with in the meridian, but a much larger and more majeſtic ſphere, the novel appearance of which ſtrikes moſt of its ſpectators with a degree of awe and amazement.

Small objects eclipſe the moon at conſiderable altitudes.

The elevated clouds are alſo large, becauſe near; and the moon ſeems ſmall, becauſe compared with them.

But in or near the horizon, the clouds, buildings, and other large objects, will appear ſmall, and hide but little of the moon's diſc; which will therefore, by compariſon, be ſeen on a grand and enlarged ſcale.

Theſe are the principal circumſtances.

Though it appears to me that the circumſtance I have pointed out, of our habitual compariſon of the moon with objects

objects of known magnitudes, has the greateſt ſhare in producing the illuſion by which it appears largeſt in the horizon, I would by no means be underſtood to reject the principal circumſtances, long ſince noticed as contributing towards the ſame effect. Different circumſtances will neceſſarily have more or leſs influence with different perſons, according to the tenor of their previouſly acquired habits and obſervations.

I remain, Sir,

Your obedient ſervant,

C. H.

Taviſtock Place, Feb. 15.

ANNOTATION. W. N.

THIS very ſtriking and perſpicuous explanation of the enlargement of the moon near the horizon, will bring to the reader's recollection a great number of facts, where the abſolute magnitudes of objects, of which the diſtances are imperfectly known, are erroneouſly eſtimated by compariſon with other objects lying in the ſame direction. Navigators muſt have been often ſtruck with the extreme minuteneſs of the image of the ſun or moon, when brought down by Hadley's quadrant ſo as to be ſeen projected upon the near objects below the horizon. Since the peruſal of this communication, I have repeated the experiment on thoſe luminaries. If the image of the moon be removed a certain number of degrees from the direct ray, it is eaſy, by altering the poſition of the quadrant, to obſerve that image in the horizon to the right or left, or upon the pavement before us, or at a great elevation in the heavens. In theſe caſes its apparent magnitude, while the angular ſize and brightneſs remain unaltered, is found to be ſmall at the great elevation, large in the horizon, and moſt ſo when projected upon diſtant objects, and it becomes a minute ſpeck when ſeen depreſſed among the objects cloſe to the obſerver. The ſame experiment may be made with a pane of glaſs, but leſs objectionably, becauſe it might be urged that the quantity of illumination is different according to the obliquity of reflection.

Experiment with Hadley's quadrant, in which the ſun and moon appear of different magnitudes, according to the objects on which their images are projected.

A Mathe-

IV.

A Mathematical Theory of the Speaking Trumpet. By JOHN GOUGH, *Eſq.*

To Mr. NICHOLSON.

SIR,

Theory of the ſpeaking trumpet.

THE ſpeaking trumpet is a conical tube, which receives the human voice from the mouth, and encreaſes the range of it. The ſtructure of theſe inſtruments, and the materials of which they commonly conſiſt, teach us to refer this ſingular faculty to two cauſes. In the firſt place, the æriel pulſes undergo certain modifications in the cavity of the trumpet; the nature and effects of which I am going to inveſtigate. Secondly, the metallic ſhell of the inſtrument augments the power of the voice, by its aptitude to conduct thoſe impulſes; which are impreſſed upon it, partly by the pulſes of the included air, and partly by the direct action of the Larynx, tranſmitted through the medium of the face to the mouth-piece of the inſtrument. Before I enter upon the inveſtigation of the effects reſulting from the firſt cauſe, it will be neceſſary to premiſe the following lemma:

Article 1. Let O R S *r* (*Fig.* IV. *Plate* 7) be an evaneſcent ſphere of air, which is agitated by two or more pulſes, beginning and ending together, and radiating from the points P *p*, &c. the vibatory motion of O R S *r*, in any direction whatever is equal to the joint ſum of the forces, which the pulſes would impreſs upon it ſeparately. Join P O, O *p*; and draw the lines R O *r*, S O *s* perpendicular to P O, O *p*; now as theſe lines are evaneſcent, every point of each of them is equally preſſed at any inſtant by its reſpective pulſe. But the force imparted to R O *r* by P, agitates the ſphere O R S *r* equally in all directions, *Principia*, 422; the ſame holds true of the force of *p* upon S O *s*. Now theſe forces cannot coaleſce or be compounded ſo as to act in a ſingle direction,—*Manch. Mem.* V. 5. p. 660; conſequently they act with their joint powers in all directions.

Art. 2. This being premiſed, let the fruſtum A B C D (*Fig.* 3.) repreſent a trumpet, the ſhell of which is a non-conductor of ſound. After a pulſe of the voice has paſſed through the aperture

aperture A B, it is bounded in front by a ſpherical ſurface, having O, the vertex of the cone D O C, for its centre, and ſubtending the plane angle A O B; in other words, let ſuch a ſurface be deſcribed any where in the fruſtum A B C D, and the variable denſity of any one particle of air in contact with it, will be equal to the cotemporary denſity of any other particle in the ſame ſurface. For as ſoon as the motion is propagated into the internal air, the pulſe will expand as far as the ſides of the tube will permit: *Princip.* 42.2: after which, the motion will be continued to the oppoſite aperture C D, in right angles, 43 *ibid.* therefore it will be propagated along the lines A D, B C with equal celerities, ſuppoſe to T and V.—On the plane T V deſcribe the ſurface T N V, in which the denſity of the air is every where equal at any inſtant: Now every point of ſuch a ſurface acts upon the air externally in contact with it, and is re-acted upon by the ſame perpendicularly to itſelf, in ſuch a manner that equal portions of the ſurface ſupport equal degrees of preſſure. Let U u be an evaneſcent particle of the curve T N V, draw U K, $u k$ perpendicular to N O; then the ſuperficial ring between the planes U K, $u k$ is as U K $\times$ U u; but this ring is as the preſſure upon it; which is alſo inverſely as the radius of curvature at U, therefore if U K $\times$ U u be conſtant, the radius of curvature is conſtant; conſequently it is equal to O T, and the truth of the propoſition is manifeſt.

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Art. 3. Let f, be the force of a pulſe in the aperture A B; and let the internal air of the cone be agitated with an equal and ſimilar impulſe at the point O; then the effect thus produced in the trumpet, will be the ſame with that of the voice acting at A B, (by *Art.* 2.) But had the cone been out of the way, the ſame force at the diſtance O A, would have been uniformly diffuſed over the ſurface of the hemiſphere, having O for its centre.

Art. 4. Put A O $= r$; the variable diſtance O T $= x$; the radius of an evaneſcent circle $= d$; the verſed ſine of half the angle A O B $= p$; 3.1416 $= c$. Then as the force f is diffuſed over the hemiſphere whoſe radius $= r$, in free air by *Art.* 3. that part of it which reſides in an evaneſcent point of the ſame ſurface $= \frac{f d^2}{2 r^2}$.

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Art. 5. The magnitude of the ſurface T N V $= 2cpx^2$: but the force f is diffuſed over this ſegment by *Art.* 3. conſequently that part of it which is confined to an evaneſcent ſpace $q = \frac{fq}{2cpx^2}$

Art. 6. Since the ſpherical ſurface T N V is convex to the plane C N D (*Art.* 2.) the agitation of the air in the latter will commence at the centre N, and extend thence by the application of ſucceſſive circles of the ſphere T N V to equal circles of the plane C D, having N for their centre; which operation will continue until T and V fall upon C and D.—Now as C N D is in contact with unconfined air, each point of it, upon being ſtruck, will become a centre, from which a pulſe will radiate freely. (*Princip.* 42.2.)

Art. 7. Let P be ſuch a phyſical point, and let its area $= q$; then if an evaneſcent plane $= q$ be drawn through P perpendicular to O P or x, the force impreſſed upon it by the trumpet will be $\frac{fq}{2cpx^2}$ (*Art.* 5) but the ſame force is imparted at the ſame time to the equal phyſical point P in the plane (*Art.* 1.)

Art. 8. Let L be the place of an ear, in O N produced, or more properly of a minute ſphere of air. Put N P $= y$; P L $= w$. Now the pulſe proceeding from P will have due effect upon the point L (*Manch. Mem.* v. V. p. 662. cor. 1); which effect is as $\frac{fq}{2cpx^2w^2}$ (*Arts.* 6 and 7): hence we have (by *Art.* 4) as $\frac{f}{r^2} : \frac{fd^2}{2r^2} :: \frac{fq}{2cpx^2w^2} : \frac{fqd^2}{4cpx^2w^2}$, for the force acting upon the ſphere L having d for its radius. But the number of points which act together $= \frac{2cyy'}{q}$ (*Art.* 6.); conſequently their united forces $= \frac{2fd^2yy'}{4px^2w^2}$. (*Art.* 1.)

Art. 9. Put L N $= g$; N O $= e$; O C $= a$; C L $= b$; and the correct fluent of the preceding expreſſion is $\frac{fd^2}{4\times pg^2-pe^2}$ drawn into the hyp. log. of $\frac{ga}{eb}$; when g and e are unequal. But if C D biſect O L; put C N $= k$, and the correct fluent becomes $\frac{fd^2k^2}{4pg^2b^2}$.

Art. 10.

Art. 10. Though the foregoing fluents have received one correction, each of them requires a ſecond, ariſing from certain circumſtances peculiar to this problem. For it is evident, that the commencement of the agitation at N is prior to that at C, by a part of time, which is as C O − O N; conſequently the completion of the former pulſe at L, will precede the termination of the latter at the ſame point by a ſimilar interval; which is as L C + C O − O L. Put this time $= t$, and the duration of one vibration of the larynx $= l$; alſo let the indiviſable interval, or $\frac{1}{300}$ of a ſecond according to Euler be denoted by m; then multiply the expreſſions in *Art.* 9 by the fraction $\frac{m-t}{l}$. Theory of the ſpeaking trumpet.

Art. 11. The force determined in the preceding paragraphs, is that which a non-conducting tube imparts to an evaneſcent ſphere of air. This circumſtance affords a plauſible objection to the preſent theory; becauſe the ſenſe of hearing is not confined to a phyſical point. But as the ſeat of this ſenſation is of a given extent, at leaſt in each perſon, the number of theſe vibrating points, in contact with the ſenſitive ſurface, is alſo given: from this it follows, that the effect of one particle is nearly a true meaſure of the power of the whole number, when the ear lies at a great diſtance from the plane C N D, repreſenting the trumpet's mouth.

Art. 12. So much of the preſent ſheet is occupied by the conſideration of the firſt cauſe, that my remarks on the ſecond, or the effects of the metallic ſhell, muſt be confined within narrow limits; theſe I ſhall begin as follows.

Art. 13. Since the conducting power of the trumpet is given, the vibrations communicated to the ſmall end of it by the breath and face, will paſs to the oppoſite extremity in a given time; which call L. Alſo let h be the force of a vibration at the diſtance 1; and C L or b expreſs the ſpace betwixt the trumpet and a remote ear.

Art. 14. In the firſt place, let l be equal to L; then a ſingle pulſe of the trumpet will ſtrike the ear for every vibration of the voice; and the force of it will be $\frac{h\,m\,d2}{L\,b^2}$ by my former paper on this ſubject. In the next place, let l be an aliquot part of L; then the number of pulſes impreſſed on the ear in

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the time of L is $\frac{L}{l}$; hence the force $\frac{h\,m\,d^2}{l\,b^2}$. Add this equation to the former of thoſe found in *Art.* 9; and the ſum gives the force of the trumpet upon a diſtant ear.

Art. 15. But if l be an aliquant part of L, the ſound will be imperfectly formed; becauſe the quantity $\frac{L}{l}$ being a mixed number, the vibrations of the larynx will be conducted along the metal, and diſcharged into the air in a diſorderly manner, ſo as to deſtroy the continuity of the note. The ſame cauſe, in all probability, conſtitutes the difficulty which the inexperienced find in their firſt attempts to ſound horns and military trumpets: for the lips, which are compreſſed by the end of the tube, form the mouth piece in this caſe; and the art of producing the ſound ſeems to conſiſt in cauſing the edges of them to vibrate in aliquot parts of L, or the interval belonging to the loweſt note of the inſtrument. The ſame origin may alſo be aſcribed to the muſical ſcale, compoſed of what are called the trumpet notes; the intervals of which decreaſe in the following ratios, 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. for $\frac{1}{2}$ is the greateſt aliquot part of unity, $\frac{1}{3}$ the next in ſucceſſion, and ſo on.

Art. 16. If the remarks contained in the two preceding paragraphs be juſt, the perſon who makes uſe of a ſpeaking trumpet, muſt attend to the management of his voice; the pitch of which ought to coincide with ſome note in the trumpet ſcale of his inſtrument: for proper attention to this circumſtance will make the ſound of the metallic ſhell perfect, and bring it into uniſon with the other ſound; which ariſes from the ærial pulſes, paſſing through the cavity of the tube, at the ſame inſtant. In this manner, the greateſt ſound will be produced, ſuppoſing the power of the voice to be conſtant; becauſe the two ſets of pulſes, conſtituting the uniſon, will impreſs the greateſt poſſible force upon the ear in a given time. On the other hand, if the foregoing precaution be neglected, the imperfect ſound of the metal will diſturb the uniformity of the other, which is formed in the cavity; and the force impreſſed on the ear by ſo diſorderly an aggregate of pulſes, will be interrupted and inconſtant.

Art. 17. Perhaps it is unneceſſary to remark, that the perfect elaſticity of the air forms the baſis of the preſent theory; a ſuppoſition

ſuppoſition which in all probability is not altogether juſt; but, when the ſmallneſs of the angle O L C is recollected, the error ariſing from the hypotheſis vaniſhes in a great meaſure.

JOHN GOUGH.

Middleſhaw, Feb. 8, 1805.

V.

Obſervations on Baſalt, and on the Tranſition from the vitreous to the ſtony Texture, which occurs in the gradual Refrigeration of melted Baſalt; with ſome geological Remarks. In a Letter from GREGORY WATT, *Eſq. to the Right Hon.* CHARLES GREVILLE, *V. P. R. S. From the Philoſophical Tranſactions for* 1804, *p.* 279.

(Concluded from Page 126.)

A CURIOUS diverſity may prevail in the products of a compound body ſubjected to fuſion, when abſolute ſolution is produced. When merely ſimple fuſion takes place, the aggregation of the parts only is deſtroyed: the fluidity ariſes from the facility with which they move on each other; and a regulated diminution of temperature, by facilitating their re-union, can hardly fail to recompoſe the ſame ſpecies that formerly appeared to exiſt in the compound. But, if the molecules have been diſſolved and decompoſed, and their component particles diffuſed through the fluid, there ſeems to be very little probability that any re-union ſhould compoſe the ſame molecules. It is more likely that new compounds will be formed, from which new molecules, and of courſe new cryſtals, will be generated; and that, conſequently, the ſame rock may become the parent of very diverſified offſpring. Theſe will however retain ſome traces of their origin; for, as there can be no fuſion of a compound body imagined, in which the mutual action of the components will not decompoſe ſome portion, there can be no ſolution ſuppoſed ſo perfect that every molecule ſhall be deſtroyed. In the firſt caſe, there will exiſt the germs of a new compoſition; and, in the ſecond, there will remain the relics of the old.

Fuſion and cooling may produce different reſults in compound bodies, the more powerful the one and gradual the other.

If theſe obſervations are correct, conſiderable utility ſeems derivable from them, in the explanation of ſome geological problems.

The aqueous formation exhibits ſimilar facts.

problems. It will appear, that they ſtrikingly illuſtrate the analogy which exiſts between the aqueous and igneous formations, and ſhow that preciſely the ſame order and kind of arrangement is followed, in the generation of ſtony maſſes from water as from fire; for, the change of ſtructure, which I have obſerved to be the moſt inexplicable part of the proceſs by which glaſs paſſes into ſtone, is almoſt exactly imitated in the formation of calcareous ſtalactites. Succeſſive depoſitions of calcareous carbonate, form a ſtalactite, which at firſt is fibrous. A continuance of the proceſs cauſes the fibrous ſtructure to diſappear, and the ſtalactite becomes irregularly ſpathoſe. The irregularities then vaniſh, and it becomes perfect calcareous ſpar, diviſible into large rhomboids, with the form peculiar to that mineral; and all the gradations may be found in the ſame ſpecimen. Nor is this change confined to a few ſolitary ſpecimens; for a conſiderable extent of coaſt near Sunderland, is formed of a lime-ſtone compoſed of radiated ſpheroids, from half an inch to three inches diameter, imperfectly united. When one of theſe ſpheroids attains ſomething more than the uſual magnitude, it becomes compact in the heart; and it is not unuſual to diſcover portions of the rock, in which the radii have entirely diſappeared, and the whole maſs has become compact. It is probable that the entire formation of oolithi and piſolithi is owing to the ſame cauſe; and that they are prevented from ever arriving at great ſize, by the union of their ſurfaces, and their ſubſequent conſolidation into compact limeſtone, into which they are continually found to graduate.

Calcareous ſtalactites: at firſt fibrous, then irregularly ſpathoſe, then ſpar.

Coaſt near Sunderland ſo formed.

Hitherto, I have ſelected inſtances from ſubſtances which have an undiſputed claim to an aqueous origin. I ſhall now, on the authority of Dolomieu, inſtance a ſimilar arrangement, in a ſubſtance reſpecting the origin of which theoriſts are not agreed. A ſpecies of petroſilex is found in the Val de Nido, in Corſica, which contains radiated petroſiliceous glands, from half a line to an inch in diameter. Theſe glands only differ from the baſis by their radiated ſtructure, and their colour; and appear to indicate very clearly, that the rock was ſubjected to a ſpecies of arrangement which, if it had been completed, would have changed its nature, and probably would have rendered it porphyritic; for Dolomieu obſerves, that the centre of the glands was often occupied by a ſmall cryſtal of feldſpar.* The extra-

Petroſilex, of doubtful origin, ſimilarly arranged in its ſtructure.

* Dolomieu. Journal de Phyſique, 1794, page 260.

ordinary rock called the globular Granite of Corſica, is an analogous inſtance. It is compoſed of cryſtals of hornblende, feldſpar, quartz, and mica, in confuſed aggregation; and in this baſis are immerſed ſpheroids, about an inch and a half or two inches in diameter, compoſed of concentric alternate coats of quartz and hornblende. The centre is principally occupied by hornblende; this is ſurrounded by a zone of quartz. Theſe ſpheroids are radiated to the centre. There can be little doubt that this rock is merely the reſult of interrupted cryſtallization; and that, if the proceſs of arrangement had continued, this ſtructure would have diſappeared, and the whole rock would have reſembled the preſent baſis. Hitherto, this very ſingular rock has only been found in detached fragments.*

Globular granite at Corſica.

The admiſſion that ſolution is not a requiſite of cryſtallization, appears to me an important conceſſion in favour of the aqueous ſyſtem, which has laboured under very great embarraſſment, from the difficulty of diſſolving quartz. If a very perfect mechanical ſuſpenſion be all that is requiſite, we may

Solution not being indiſpenſable to cryſtallization, but only mechanical ſuſpenſion, many geological difficulties are removed.

* I ſhall venture to quote another inſtance, on the authority of Profeſſor Playfair. "The ſalt rock in Cheſhire, which lies in thick "beds, interpoſed between ſtrata of an argillaceous or marly ſtone, "and is itſelf mixed with a conſiderable portion of the ſame earth, "exhibits a very great peculiarity in its ſtructure. Though it "forms a maſs extremely compact, the ſalt is found to be arranged "in round maſſes, of five or ſix feet in diameter, not truly ſpherical, "but each compreſſed by thoſe that ſurround it, ſo as to have the "ſhape of an irregular polyhedron; theſe are formed of concentric "coats, diſtinguiſhable from each other by their colour, that is, "probably, by the greater or leſs quantity of earth which they con"tain; ſo that the roof of the mine, as it exhibits a horizontal ſec"tion of them, is divided into polygonal figures, each with a mul"titude of polygons without it, having altogether no inconſider"able reſemblance to a Moſaic pavement. In the triangular ſpaces "without the polygons, the ſalt is in coats, parallel to the ſides of "the polygons." Illuſtration of the Huttonian Theory, page 37.

I am informed, that the ſiliceous depoſition at Geyſer, is at firſt a porous friable maſs, and that the addition of more molecules renders it fibrous; alſo that, on a farther addition, the fibrous ſtructure diſappears, and the whole aſſumes the compact even texture of chalcedony or flint. If I am not miſinformed, a ſeries of ſpecimens illuſtrating this tranſition, exiſted in the cabinet of the late Dr. Hutton, of Edinburgh.

ceaſe

ceaſe to wonder at the almoſt daily formation of petrified wood, (in which, though cryſtallization does not actually take place, a very perfect arrangement is indicated, by the intimate union of the ſiliceous particles,) or of hydrophanous ſemi-opals in the decompoſed ſerpentine of Muſſinet, near Turin, or of chalcedony containing drops of water, in the decompoſed baſalt of Vicenza.

Since the cryſtallizing may not take place in the order of fuſibility, refractory ſubſtances may be impreſſed by ſuch as are more fuſible.

I have endeavoured to ſhow, that in the cryſtallizations reſulting from igneous fuſion, it is not only poſſible but probable, that the moſt infuſible ſubſtances might not be the firſt to cryſtallize; and this appears to involve important conſequences, for it partly removes one of the greateſt difficulties that embarraſſes the igneous theory, by explaining the poſſibility of refractory ſubſtances generated by fire being impreſſed by the forms of more fuſible ones. It ſeems, however, that the ſame order of arrangement would prevail in ſubſtances that were ſuſpended in a fluid medium, as the degrees of attraction would be the ſame. In either caſe, the firſt ſtep by which the arrangement of an apparently homogenous maſs commenced, would probably be the accumulation of particular molecules into little globules. Such ſeems to have happened in variolites, and other rocks which contain ſpherical concretions of a different nature from their baſis. Still farther advanced is the arrangement of porphyries: the molecules of one ſpecies have aſſumed a regular cryſtalline form; and ſometimes two or even more varieties o cryſtals are formed, which remain unmixed in the unarranged baſis. If the remaining molecules of that baſis are ſuſceptible of cryſtallization, it may be fairly concluded, that an extenſion of the proceſs of arrangement would convert the porphyry into granite, or at leaſt into one of the compound aggregates of cryſtals which conſtitute the numerous tribes of granites, grunſteins, and ſienites; and it ſeems equally probable that this might be accompliſhed, whether the molecules were indebted to a ſuitable temperature, or to an aqueous medium, for the requiſite facility of movement.

Inſtances and remarks.

Granites and porphyry.

The formation of granite and other rocks, muſt however be referred to the ultimate perfection of cryſtallization, by which all the molecules have been permitted to arrange. Thoſe granites called porphyritic, in which large cryſtals of feldſpar are imbedded in a baſis compounded of the ordinary ingredients of granite in ſmall grains, are apparently generated from a menſtruum

ſtruum in which the molecules of one ſpecies, being greatly predominant in number to the reſt, are the firſt to exerciſe their polarity, and conſtitute large cryſtals, which are afterwards ſurrounded by ſmaller ones, reſulting from the ſucceſſive ſeparations of the remaining elementary molecules.

The changes of the ſubſtance that led to the foregoing remarks, ſerve to ſhow that they are not altogether hypothetical; and any proof that may appear deficient, ſeems to be provided by the phenomena exhibited by lavas, in which may be obſerved every ſtep of the paſſage from the vitreous to the ſtony, from that to the porphyritic, and finally to the granitic ſtate. The lava of Lipari, which paſſes from glaſs to lava, by the generation of minute globules, may be cited, on the authority of Spallanzani, as an inſtance of the commencement of the proceſs of arrangement; * and, were not their origin ſtill diſputed, I might alſo cite the pitchſtone lavas of the Euganean hills. It would appear, that the tranſition from the ſtony to the porphyrytic ſtate is rapid, for perfectly homogeneous lavas are among the rareſt of volcanic products. The porphyritic lavas are moſt numerous; and it is needleſs to detail the varieties they preſent.

The paſſage from the vitreous to the ſtony ſtate, from that to the porphyritic, and laſtly to the granitic ſtate are obſervable in lavas.

* Spallanzani, *Viaggi alle due Sicilie. Tomo Secondo*, page 238. The whole paſſage, literally tranſlated, ſtands thus: "This lava "has a baſis of feldſpar, of a fine and compact grain, a ſplintery "fracture, rough to the touch, and emitting ſparks, like flint, "when ſtruck with ſteel. It has an aſh colour, in ſome places ap- "proaching to a leaden colour. It is thickly filled with an immen- "ſity of little bodies, which would be diſtinguiſhed with difficulty, "from the reſemblance of their colour to that of their baſis, were "it not for their globular form. But this lava is joined to a great "maſs of glaſs, which forms a whole with it, without any diviſion "or ſeparation between them; and this lava, which in many places "retains its own nature, is in many other places reduced to glaſs. "Some parts of this glaſs are filled with the ſame little bodies, but "other parts are pure glaſs. This is in general very compact, has "a dead black colour, and breaks rather into irregular pieces than "into undulated fragments, as glaſs properly does. Beſides, it has "I know not what of unctuoſity to the touch, and to the eye, "which is not perceptible to the more perfect volcanic glaſſes. It "yields ſparks with ſteel, like the lava; but the lava is wholly "opaque, and the glaſs, at the angles and thin edges, has conſider- "able tranſparency. It is only opaque where the globules are, "which appear to be particles of lava."

But,

But, though the proceſs of arrangement has often only advanced thus far, it has in many inſtances proceeded much farther, and it is by no means unuſual to find the entire baſis regularly arranged into cryſtalline bodies; thus, to cite a well known inſtance, in many of the ancient lavas of Somma, large augites are imbedded in a cryſtalline maſs, formed of minute cryſtals of leucite, together with another cryſtalline ſubſtance, whoſe nature is not perfectly determined.

Volcanic glaſs.

The caſual occurrence of volcanic glaſs is nowiſe at variance with this account, as it is ſufficiently probable, that ſome glaſſes may have a much greater tendency to cryſtalline arrangement than others poſſeſs; and it cannot appear extraordinary, that regular cryſtals ſhould ſometimes be generated, even in the glaſs, as it is a matter of daily occurrence in artificial glaſſes, and in furnace flags.

Peculiar bodies may be afforded by this method of ſeparation.

If the diſtinction attempted to be ſhown between igneous fuſion and ſolution be eſtabliſhed, it may offer a means of accounting for the abundance of peculiar bodies in lava, which do not exiſt in other ſituations, or at leaſt are of extremely rare occurrence. For, if the igneous action decompoſes the molecules of the ſubſtances on which it operates, there ſeems every probability that new compounds may reſult, diſſimilar to any ſubſtances we are acquainted with. It would appear, that the neceſſity of imagining an undiſcovered ſtratum abounding in leucites, chryſolites, and augites, may be diſpenſed with; and, as I have endeavoured to ſhow the probability that the moſt infuſible ſubſtances will not be the firſt to cryſtallize, the penetration of refractory leucites by fuſible augites, will ceaſe to be an argument againſt both being generated in the lava. I may alſo obſerve, that the ſame cauſes which vary the cryſtallized bodies reſulting from igneous ſolution, muſt operate upon the unarranged baſis; and that the ſame rock may be fuſed into lavas extremely diſſimilar, as their varieties muſt depend on the degree of ſolution which the fuſion has accompliſhed.*

The facts and doctrines here given will not affect the queſtion of the igneous or aqueous formation of baſalt.

If the analogy attempted to be ſhown between the aqueous and igneous formation appear founded, the tranſition from glaſs to

* The evidence of the generation of leucites in the lava which contains them, collected by Leopold de Buch, and Breiſlac, and finally acquieſced in by Dolomieu, appears ſo ſatisfactory, that it can hardly be deemed preſumptuous to aſſume the point as determined.

to ſtone can no way affect the great queſtion which has ſo long divided geologiſts, about the origin of baſalt; for though it is ſynthetically demonſtrated that baſalt may be formed by fire, the converſe of that propoſition ſtands ſupported by ſtrong analogical arguments, and its formation by water muſt be allowed to be at leaſt equally poſſible. How far the probabilities derived from the examination of baſaltic formations may influence the ultimate deciſion, is an enquiry in which I ſhall not now engage; though I cannot avoid recalling to my mind, the numerous inſtances of petrifactions found in baſalt, and, as a counterpoiſe to that obſervation, the equally numerous inſtances in which the heat emanating from it appears to have indurated ſtrata, and coaked beds of coal. One remark may be ſtated here with propriety, as it ariſes immediately from the experiment which has occaſioned theſe obſervations. In the ultimate reſult of that experiment, the arrangement of the molecules was much more perfect than in the original rock. It might be ſuppoſed, that a longer continuance of the ſuitable temperature was afforded it. This, however, could not be, for the maſs was only a few feet long, and a few inches thick; the fire was only maintained a day; and the whole was cooled in a week. But the hill of ſolid baſalt, from which the ſubſtance operated upon was taken, is ſeveral miles long, and ſeveral hundred feet high; and, ſuppoſing it to have been irrupted in a ſtate of igneous fuſion, it muſt have required months, nay years, for its refrigeration. How then comes it, that the proceſs of cryſtallization is ſo little advanced? How comes the confuſion of its texture to indicate the very reverſe of the tranquillity and perfection of arrangement, which may be fairly aſſumed as neceſſarily attending the extremely gradual changes of ſo immenſe a maſs?

Why ſo little of cryſtallization appears in the ſtructure of baſalt.

This objection admits of being obviated, upon the ſuppoſition that, in the proceſs of melting, the molecules of the baſalt were decompoſed; and that the new ones generated were more diſpoſed to cryſtallize than thoſe whoſe place they ſupplied. This explanation is in ſome degree juſtified, by the

Probably becauſe new molecules were formed more diſpoſed to cryſtallize.

mined. Neither de Buch nor Dolomieu have been able to convince themſelves that the augites were alſo formed in the lava; but I confeſs myſelf entirely unable to appreciate the cogency of their arguments, which ſeem annihilated by the admiſſion they have made in favour of leucites.

total

total diſappearance of the minute fieldſpar and hornblende of of the baſalt; inſtead of which, the regenerated ſtone contains thin laminæ of cryſtals, which are probably augites.

The diviſion of baſalt into globular maſſes during its decompoſition, indicates the formation of radiated ſpheroids therein;

I cannot leave this ſubject without noticing ſome particulars, in which the proceſs of arrangement deſcribed in the early part of this letter, appears to yield a probable explanation of ſome of the peculiarities of baſalt. The general diſpoſition of baſalt to divide into globular maſſes, in decompoſing, is too remarkable a fact to have eſcaped the attention of naturaliſts; though, as far as I am informed, no ſatisfactory explication of it has been given. The common effects of decompoſition are obviouſly inadequate; for it is common to ſee a large block of amorphus baſalt ſeparate into numerous balls, after a few months or years expoſure to the weather; and, rapid as the proceſs of decompoſition has been in the intervening portions, theſe balls reſiſt its father progreſs with uncommon obſtinacy. May not this be attributed to the formation of the radiated ſpheroids, whoſe occurrence in my experiment I have already mentioned? and may not their greater reſiſtance of weather ſimply ariſe from their aggregation being more perfect than that of the incoherent molecules which have filled the intervals between them? Though the radiated ſtructure has diſappeared to the eye, theſe portions of the ſtone retain the ſuperiority of more perfect internal arrangement; and, if my pigmy experiments could yield ſpheroids of two inches diameter, there can be no difficulty in ſuppoſing that the grand operations of nature may produce them of ſeveral feet. The ſeparation of the decompoſed fragments in concentric coats, ſeems eaſily explained; for I have already pointed out the facility with which the radii of the ſpheroids ſeparated at nearly the ſame diſtances from their centres, and the form of the fragments which reſulted, reſembling fragments of bombs. *

and theſe may have naturally formed the baſaltic columns.

If this idea be not conſidered as entirely diveſted of plauſibility, I may venture to extend the ſame principle, to account for the wonderful regularity of the priſmatic configuration of

* Even granite has been frequently obſerved to affect globular decompoſition, and diviſion into fragments of concentric coats. This mode of decompoſition extends to many ſubſtances, that Werner has called the formation it ſeems to indicate, "*abgeſonderte ſtücke*," which has been rendered in Engliſh, *diſtinct concretions.*

baſaltic

baſaltic columns, and alſo for their articulations. If we ſuppoſe that a maſs of fluid baſalt has filled a valley to an indefinite depth and extent, the proceſs of arrangement in its particles muſt be induced by the removal of its heat or moiſture, according as its ſolution is igneous or aqueous. This can only be done by the action of the atmoſphere on its upper ſurface, and by the ground on which it repoſes abſorbing the heat or moiſture from its under ſurface. From the variations of the atmoſphere, its action muſt be irregular; and, from the perpetual change of the parts in contact with the heated or moiſt ſurface, its operations will always be nearly as active as at firſt, allowance being made for its variations. But the abſorption of the ground will be regular, and regularly diminiſhing in activity, in proportion as the parts near the maſs approach nearer to the ſame temperature, or ſame moiſture, with the maſs above; and its abſorptions can only be carried on by its tranſmiſſion of heat or moiſture to the ſolid rocks below. From theſe conſiderations it ſeems evident, that the arrangement of the part of the baſalt near the ground, will be begun with more energy than it can be continued, and that the reſults will be more ſlow and regular than the arrangement induced by the perpetual though variable action of the atmoſphere. After the firſt ſtage in the proceſs of arrangement has been performed, and a ſtratum, if I may ſo term it, of the jaſpedeous ſubſtance extended over the ſurface of the ground, there ſeems no reaſon to doubt that a number of radiated ſpheroids would be generated in it, which would probably have all their centres about the ſame diſtance from the ground; and, as the arranging power undergoes a gradual diminution of energy, it is not probable that two rows in height of them ſhould be formed at once, as that would indicate a haſty proceſs, which had prepared a greater maſs of matter for their almoſt ſimultaneous formation. From theſe conſiderations, there ſeems no improbability in ſuppoſing, that in the arrangement of a maſs of fluid baſalt, a ſingle layer of radiated ſpheroids would be formed, repoſing on the ground which ſupported the maſs.

Enumeration of facts and circumſtances.

1. Radiated ſpheroids formed near the ground,

in one layer.

I have already ſtated, that when the radii of two ſpheroids came into contact, no penetration enſued, but the two bodies became mutually compreſſed, and ſeparated by a plane, well defined, and inveſted with a ruſty colour. I alſo ſtated, that when ſeveral ſpheroids encountered, they formed one another

2. Theſe would take hexagonal peripheries at the places of ſide contact;

into prifms with well defined angles. In a ftratum compofed of an indefinite number in fuperficial extent, but only one in height, of impenetrable fpheroids, with nearly equidiftant centres, if their peripheries fhould come in contact on the fame plane, it feems obvious that their mutual action would form them into hexagons; and, if thefe were refifted below, and there was no oppofing caufe above them, it feems equally clear, that they would extend their dimenfions upwards, and thus form hexagonal prifms, whofe length might be indefinitely greater than their diameters. The farther the extremities of the radii were removed from the centre, the nearer would be their approach to parallelifm; and the ftructure would be finally propagated by nearly parallel fibres, ftill keeping within the limits of the hexagonal prifm with which their incipient formation commenced; and the prifms might thus fhoot to an indefinite length into the undifturbed central mafs of the fluid, till their ftructure was deranged by the fuperior influence of a counteracting caufe, which would be provided by the action of the atmofphere on the upper furface of the bafalt. If this arrangement exifted, the fame caufe that determined the concentric fractures of the fibres of the fperoids, would produce convex articulations in the lower joints of the prifms; and, in proportion as the centre from which they were generated became more remote, the articulations would approximate to planes. If the generating centres were not equidiftant, the forms of the pillars would be irregular; and the irregularity would be in proportion to the diverfity of diftance between the centres. If the difference was great, the number of fides would be altered, and they might be found pentagonal, tetrahedral, and trihedral. As the compreffion of the fibres would be greateft in the level of the generating centres, the lower part of the prifms would be moft compact.

and other fpheroids formed above them would produce articulated columns.

All thefe conditions feem to be fulfilled, in the actual formation of bafaltic columns; for, in every inftance I am acquainted with, they appear to have been formed in the tranquil bofom of the mafs, as they have been originally maſked by amorphous trap, and their prifmatic ftructure is only difplayed by the removal of this covering. This has been variouſly effected, ſometimes by the apparent difrupture of rocks, ſometimes by the exterior portions of the mafs being thrown

The facts attending the production of bafaltic columns agree with the foregoing.

thrown down by the failure of the ground on which it ſtood, ſometimes by the violence of the waves, and not unfrequently by the working of quarries. In moſt inſtances, theſe operations have only removed the covering from one ſide of the colonnade; and it remains crowned, and generally ſurrounded, by an immenſe amorphous maſs. Where there are two ranges of columns, with an intervening amorphus ſtratum, it is probable that the upper is the reſult of a ſecond inundation of fluid baſalt. It is well known that baſaltic columns are moſt ſolid at the bottom; and their convex articulations have been repeatedly obſerved. Since theſe conſiderations occurred to me, I have had no opportunity of examining, whether the diviſions approach nearer to the plane ſurfaces as they recede from the centre from which the priſms was generated, nor whether below that centre the convex ſurface of the articulations is inverted; but I think it by no means improbable, that ſubſequent obſervations may eſtabliſh this to be the caſe, and thus confer on this hypotheſis nearly all the demonſtration of which it is ſuſceptible. I may however add, that the phenomena of priſmatic diviſion in baſaltic veins, perfectly coincide with what might be inferred from the data upon which my reaſoning has proceeded. In veins, it is obvious that the refrigerating or abſorbing cauſe muſt operate with nearly equal force on each ſide of the vein; and it follows, that two ſets of priſms would be generated, which would be horizontal inſtead of perpendicular, and that, unleſs a maſs of amorphous baſalt was interpoſed between them, they muſt form a diviſion in the middle of the vein, as, from the mutual impenetrability of their fibres, they could not incorporate. The coincidence of the exiſting phenomena with theſe concluſions, is ſufficiently remarkable; for, in numerous obſervations I have made on the baſaltic veins which effect the priſmatic configuration, I found the priſms were always horizontal, and often, that there were two ranges of them. One of their ends applied to the wall of the veins, the other frequently united to an amorphous maſs which ſeparated them; and, when no ſuch intermedium occurred, there was invariably a diviſion in the middle of the vein. Not unfrequently, the veins contain three ſets of priſms; a range of ſmall ones on each ſide, and of much larger ones in the middle. In this caſe, the little priſms

Baſaltic veins.

are

are always ſeparated from the large ones, and the diviſions of the large ones are very irregular. *

These deductions do not ſuppoſe either the aqueous or igneous formation of baſaltes to be eſtabliſhed.

After the ſtatement of my opinion, that perfect ſimilarity of ſtructure may exiſt in the products of aqueous and igneous formation, it will hardly be neceſſary to conclude theſe obſervations with remarking, that I ſhould not conſider the eſtabliſhment of theſe peculiar modes of arrangement as the ſlighteſt demonſtration of the igneous origin of baſalt. It appears to me, that the truth of my deductions is entirely independent of either theory, and that, if ever the period ſhould arrive when the origin of baſalt ſhall be determined by irrefragable demonſtration, the inferences I have drawn may be accommodated with equal facility to either mode of agency. †

The

* The obſervations alluded to were made during the courſe of laſt ſummer (1803), on the very numerous baſalt veins, or, as they are there called, Whin Dykes, which traverſe the red ſandſtone and red ſandſtone breccia, which forms the greateſt part of the coaſt of the Firth of Clyde, between Greenock and the Largs.

† Mr. Keir, in his paper on the cryſtallizations formed in glaſs, ſuggeſts the probability of baſaltic pillars being formed by the cryſtallization of vitreous lavas. See Philoſophical Tranſactions for 1776, Vol. LXVI. page 530.

Dolomieu was of opinion, that the priſmatic form was peculiar to lavas which had flowed into the ſea; and he attributed it to the ſhrinking of the maſs: his deſcription of the appearances exhibited by what he calls the priſmatic lavas at the foot of Etna, merits quotation.

" In the lavas of Etna, the form and dimenſions of the columns vary as much as the manner in which they are grouped; hexaedral and pentaedral priſms are moſt abundant; then the tetraedral, the triedral, heptaedral, and octaedral. The leaſt I have ſeen are only four inches diameter; others are more than three feet; they are commonly of a ſingle ſhoot, which is ſometimes 60 feet high; others are divided by articulations, which are from one to ſix feet aſunder.

" I have more than once obſerved a large column divide into ſeveral ſmaller in its upper part. The columns are generally larger near the top than the bottom of the ſtream of lava, becauſe they ſubdivide; and they are always leaſt in that part of the ſtream of lava which firſt entered the water, the refrigeration being more

prompt,

The immenſe magnitude of ſome baſaltic columns, the extreme regularity of their priſmatic configuration, and the peculiar ſtructure of their articulations, have directed the attention of naturaliſts to them, much more than to any of the other rocks which affect the columnar form. Yet many of theſe are ſufficiently remarkable to deſerve more particular notice than has generally been paid to them; and they afford moſt illuſtrative proof, that this configuration is not confined to either the aqueous or igneous formation; for ſome lavas, univerſally allowed to be ſuch, are priſmatic.* Columns of porphyry

Other rocks alſo affect the columnar form.

prompt, and its effects more marked. Sometimes the columns are placed perpendicularly ſide by ſide, and form vertical walls, which are ſometimes more than 100 feet high, and a league long; ſometimes they are heaped obliquely, horizontally, and in all poſitions. Some, without being divided in their length, are larger at one end than the other; and then they are arranged like wood piled up, with all the ſmall ends at one ſide; ſometimes they are formed into pyramidal bundles, by parting from a common center; and, finally, there are ſome which, by their reunion, form large balls. Theſe radii of lava, which are rather pyramidal than priſmatic, reſemble thoſe of the globular pyrites, ſtriated from the center to the circumference, which are found in the chalk of Champagne.

"On the ſhore of la Trezza, near the mole, there is a very curious group of little articulated priſms, which iſſue from a common center, and form faſciculi ſingularly twiſted. The articulations are marked, but the ſpecies of vertebræ do not ſeparate. In the heart of the mountain on which ſtands the Caſtello di Jaci, there are large balls, from two to four feet in diameter, reſembling in form the large pyrites in the chalk of Champagne. Theſe balls of lava are formed of pyramidal columns, united by their points in a common center." *Catalogue des Laves de l'Etna*, page 453.

The diviſion of the upper part of baſaltic columns into ſeveral ſmaller ones, has alſo been obſerved in the baſaltic columns of Fairhead, by Dr. Richardſon. See Nicholſon's Journal, 4to. Vol. V. page 321.

* Almoſt all the priſms at the foot of Etna, deſcribed by Dolomieu, are of dubious origin; moſt of them are probably baſalt. The columns of the Vincentine are of the ſame ſubſtance, and ſo are the priſmatic lavas of Auvergne, and of the Vivarais. The bed of lava at la Scala, near Portici, is divided by vertical fiſſures, which give it the aſpect of irregular columns. At Aquapendente, in a quarry of undoubted lava, near the road, are ſome much more

Inſtances: Porphyry, petroſilex, argillaceous ſchiſtus, rubble ſlate, limeſtone, &c.

porphyry are not rare; and, among other places, are found near Dreſden, ſeveral feet in length, and not more than two inches in diameter. Columns of petroſilex compoſe a large portion of a mountain near Coniſtone lake. Very perfect quadrangular priſms of argillaceous ſchiſtus are found near Llanwrſt. Rubble ſlate aſſumes the columnar form at Barmouth. The limeſtone near Cyſartha, in Glamorganſhire, is divided into very regular acute rhomboidal priſms: even the ſandſtone of the ſame diſtrict is not unfrequently columnar; and one of the beds of gypſum at Montmartre is diſtinctly divided into pretty regular columns. Sandſtone, clay, argillaceous iron ore, and many other ſubſtances, become priſmatic by torrefaction; and the priſms of ſtarch formed in drying have often been conſidered as illuſtrative of baſaltic formations.

Mere contraction may produce effects reſembling theſe, but leſs regular.

I am very far from conceiving, that all theſe configurations are influenced by ſuch ſyſtematic arrangements as have determined the form of ſome baſaltic columns. I conſider moſt of them as ſolely attributable to contraction; which is only a farther extenſion of the aggregative force, and muſt be regulated by the texture, the form, and the poſition of the maſs. Where the texture of the maſs is homogeneous, and its contractions uniform, its dimenſions may be diminiſhed, without its continuity being deſtroyed, provided its aggregation be ſo ſtrong as to overcome the *vis inertiæ* of the maſs, and its adheſion to other ſubſtances. But, when the reſiſtance is ſufficient to overcome the aggregation, the maſs will be rent by fiſſures perpendicular to the direction in which the greateſt reſiſtance to its contraction takes place, or, in other words, by fiſſures perpendicular to its greateſt ſurface: for it is from the extremities of the greateſt ſurface, that the largeſt quantity of

Developement of the effects of contraction in producing ſymmetric fractures.

perfect priſms; but the moſt beautiful I have ſeen, are the ſmall ones from Ponza. The columns at Boliena are ſaid to be baſalt. Thoſe of the Euganean hills are very irregular in form; in their texture they are certainly wholly unlike granite, which Mr. Strange thought they reſembled. I believe them to be lava.

The mention of ſome columnar formations that follows, is by no means intended as an enumeration of them. I have confined myſelf to thoſe which I have either inſpected in their natural ſituation, or of which I have ſeen numerous ſpecimens.

matter muſt traverſe the greateſt ſpace, in order that the contraction may be performed without breach of continuity; therefore, if it be an extenſive tabular maſs, it will be divided into priſms, by fiſſures perpendicular to its ſurfaces. The power of aggregation would determine theſe priſms to be hexagonal, as that form contains the greateſt quantity of matter in the leaſt ſurface, of any priſms that can be united without interpoſing priſms of other forms. But this would require the texture, the contraction, the thickneſs of the maſs, and its adheſion to ſurrounding ſubſtances, to be every where preciſely the ſame; and, as theſe conditions can never be fulfilled in an extenſive formation, all the irregularities that are found muſt neceſſarily enſue. The ſame rule that determines the fiſſures of a tabular maſs to be perpendicular to its ſurfaces, muſt determine the rents in a ſpheroid to be directed from its periphery to its centre.

They do not account for the regular form of baſaltes.

Though theſe conſiderations may be ſufficient to explain the tendency to diviſion into priſms, which is ſo generally extended, and which has produced many of thoſe abortions that have been dignified with the name of columns, becauſe they have occurred in lavas and in rocks of trap formation, they are utterly inadequate to illuſtrating the formation of the more perfect baſaltic priſms: they offer no means of accounting for the extreme regularity of the ſides and the preciſion of the angles, for the articulations, for the cloſe contact in which the perfect columns are placed to one another, not for their mutual adheſion, which is ſo ſtrong, that it often requires conſiderable violence to ſeparate them. Theſe facts are in abſolute contradiction to all idea of retreat or contraction, and ſeem to me to coincide perfectly with the explanation of their origin which I have already preſumed to lay before you.

I have the honour to be, &c.

GREGORY WATT.

VI.

Improved Conſtruction of WOULFE'S *Apparatus. By* W. B.

To Mr. NICHOLSON.

SIR,

Improvement in Woulfe's apparatus.

EVERY one who has been in the habit of uſing a Woulfe's apparatus, muſt have experienced the difficulty of fixing the tubes of communication, and the continual riſk of breaking them. To obviate this inconvenience, I would ſuggeſt the following mode of conſtructing the apparatus, which, if you think any improvement upon the preſent method, cannot be rendered more generally uſeful than by inſertion in your Journal.

I am, Sir,

Your very obedient humble ſervant,

W. B.

Feb. 5, 1805.

Fig. 1 repreſents one of the bottles. The bent tube, A B C, being attached to the outſide of the bottle when both heated, and entering it at C, may be made as ſtrong as is thought conſiſtent with neatneſs.

The part H B of the tube is of a conical form, ſmall enough at A to be inſerted in a perforated cork.

The ſhort tube D E, on the oppoſite ſide, is alſo of a conical form, large enough at E to admit a perforated cork.

The opening at F being intended for the tube of ſafety.

Fig. 2 repreſents the apparatus put together, the receiver being added as of a convenient form for connecting with the other veſſels.

The ſmall tube A B of one bottle, is inſerted in the perforated cork fitted to the tube D E of the preceding one, and the joinings ſecured in the uſual way by luting.

The tubes of ſafety being firſt ſecurely fixed, and the cork cloſely fitted in the tube D E, any number of bottles may be ſucceſſively added, without the leaſt riſk of deranging thoſe that are already connected.

Obſervation

VII.

Observations on the Change of some of the proximate Principles of Vegetables into Bitumen; with Analytical Experiments on a peculiar Substance which is found with the Bovey Coal. By CHARLES HATCHETT, *Esq. F. R. S. From the Philosophical Transactions for* 1804.

§ I.

ONE of the most instructive and important parts of geology, is the study of the spontaneous alterations by which bodies formerly appertaining to the organized kingdoms of nature have, after the loss of the vital principle, become gradually converted into fossil substances.

Conversion of organized bodies into fossils.

In some cases, this conversion has been so complete, as to destroy all traces of previous organic arrangement; but, in others, the original texture and form have been more or less preserved, although the substances retaining this texture, and exhibiting these forms, are often decidedly of a mineral nature. Some, however, of these extraneous fossils (as they are called) retain part of their original substance or principles, whilst others can only be regarded as casts or impressions.

The degrees are various.

From the animal kingdom we may select, as examples, the fossil ivory, which retains its cartilage*; the bones in Gibraltar rock, consisting of little more than the earthy part or phosphate of lime; the shells forming the lumachella of Bleyberg, which still possess the lustre and irideſcence of their original nacre; and the shells found at Hordwell in Hampshire, and in Picardy, which are chiefly porcellaneous, but more or less calcined; also the fossil echini and others, so commonly found in the limestone, chalk, and calcareous grit of this island, which, although they retain their original figure, are intirely, or at least externally, formed of calcareous spar, incrusting a nucleus of flint or chalcedony. And if, in addition to these, we may be allowed to regard the more recent limestone and chalk strata as having been principally or partly formed from the detritus of animal exuviæ, we shall possess a complete series of gradations, commencing with animal substances analogous in

Instances in the animal kingdom,

* I have also found the cartilage perfect, in the teeth of the mammoth.

properties

properties to thoſe which are recent, and terminating in bodies decidedly mineral, in which all veſtiges of organization have been completely deſtroyed.

and in the vegetable.

The vegetable kingdom has likewiſe produced many inſtances not leſs remarkable; and it is worthy of notice, that animal petrifactions are commonly of a calcareous nature, while, on the contrary, the vegetable petrifactions are generally ſiliceous *.

It is not, however, my intention here to enter into a minute diſcuſſion concerning the formation of theſe extraneous foſſils; I ſhall therefore proceed to conſider other equally or perhaps more important changes, which organized bodies, eſpecially vegetables, appear to have ſuffered, (after the extinction of the principle of life,) by being long buried in earthy ſtrata, and by being thus expoſed to the effects of mineral agents.

§ II.

Bituminous matters are derived from organized matters; chiefly vegetables.

The principal object I have in view, is to adduce ſome additional proofs, that the bituminous ſubſtances are derived from the organized kingdoms of nature, and eſpecially from vegetable bodies; for, although many circumſtances ſeem to lead to the opinion, that the animal kingdom has in ſome meaſure contributed to the partial formation of bitumen, yet the proofs are by no means ſo numerous, nor ſo poſitive, as thoſe which indicate the vegetable kingdom to have been the grand ſource from which the bitumens have been derived. But this opinion, (founded upon very ſtrong preſumptive evidence,) although generally adopted, is however queſtioned by ſome perſons; and I ſhall therefore bring forward a few additional facts, which will, I flatter myſelf, contribute to demonſtrate, that bitumen has been, and is actually and immediately formed, from the reſin, and perhaps from ſome of the other juices of vegetables.

Pure bitumens differ much from vegetable juices;

The chemical characters of the pure or unmixed bitumens, ſuch as naptha, petroleum, mineral tar, and aſphaltum, are, in certain reſpects, ſo different from thoſe of the reſins and other inſpiſſated juices of recent vegetables, that, had the former never occurred but in a ſeparate and unmixed ſtate, no poſitive inference could have been drawn from their pro-

* Pyrites, ochraceous iron ore, and fahlertz, are alſo occaſionally found in the forms of vegetable bodies.

perties

perties in proof of their vegetable origin. Fortunately, however, they have been more frequently found under circumſtances which have ſtrongly indicated the ſource from whence they have been derived; and much information has been acquired from obſervations made on the varieties of turf, bituminous wood, and pit coal, on the nature of their ſurrounding ſtrata, on the veſtiges of animal and vegetable bodies which accompany them, and on various other local facts; all of which tend conſiderably to elucidate the hiſtory of their formation, and to throw light upon this intereſting part of geology.

but the progreſſive changes are inſtructive in theſe reſpects.

Some inſtances have already been mentioned, which ſhow that foſſil animal ſubſtances form a ſeries, commencing with ſuch as are ſcarcely different from thoſe which are recent, and terminating in productions which have totally loſt all traces of organization.

Similar inſtances are afforded by the vegetable kingdom; but without entering into a minute detail of every gradation, I ſhall only cite three examples in this iſland, namely,

Series of change.

1. The ſubmarine foreſt at Sutton, on the coaſt of Lincolnſhire, the timber of which has not ſuffered any very apparent change in its vegetable characters *.

1. Submarine wood.

2. The ſtrata of bituminous wood (called Bovey Coal) found at Bovey, in Devon; which exhibits a ſeries of gradations, from the moſt perfect ligenious texture, to a ſubſtance nearly approaching the characters of pit coal, and, on that account, diſtinguiſhed by the name of Stone Coal.

2. Bovey coal in all its gradations.

3. And laſtly, the varieties of pit coal, ſo abundant in many parts of this country, in which almoſt every appearance of vegetable origin has been deſtroyed.

3. Perfect pit-coal.

The three examples above-mentioned, appear to form the extremities and centre of the ſeries; but as, from ſome local circumſtances, the proceſs of carbonization, and formation of bitumen, has not taken place in the firſt inſtance, and as theſe effects have proceeded to the ultimate degree in the laſt, it ſeems moſt proper that we ſhould ſeek for information, and for poſitive evidence, in the ſecond example, which appears to be the mean point, exhibiting effects of natural operations, by which bitumen and coal have been imperfectly and partially

The ſecond ſtate offers the moſt inſtructive ſubject for reſearch.

* Account of a ſubmarine Foreſt on the Eaſt Coaſt of England, by Dr. Correa de Serra. Phil. Tranſ. for 1799, p. 145.

formed

formed, without the abſolute obliteration of the original vegetable characters; and, although I have ſelected the Bovey coal as an example, becauſe it is found in this country, we muſt recollect that ſimilar ſubſtances, or ſtrata of bituminous wood, are found in many parts of our globe; ſo that the example which has been more immediately choſen, is neither rare nor partial *.

The nature, however, of the various kinds of bituminous wood, may in ſome reſpects be different; but this I have not as yet had the means of aſcertaining; I ſhall therefore only ſtate the facts reſulting from experiments made on Bovey coal, and more eſpecially on a peculiar bituminous ſubſtance with which it is accompanied. But, before I enter into theſe particulars, it will be proper to mention a very remarkable ſchiſtus, with which I was, ſome months ſince, favoured by the Right Hon. Sir Joſeph Banks.

Remarkable ſchiſtus,

§ III.

from Iceland,

This ſchiſtus was found by Sir Joſeph, in the courſe of his tour through Iceland, near Reykum, one of the great ſpouting hot ſprings, diſtant about twenty-four Engliſh miles from Hafnifiord; but circumſtances did not permit him to aſcertain the extent of the ſtratum.

conſiſting very much of alder leaves.

The ſingularity of this ſubſtance is, that a great part of it conſiſts of leaves, which are evidently thoſe of the alder, interpoſed between the different lamellæ. I do not mean mere impreſſions of leaves, ſuch as are frequently found in many of the ſlates, but the real ſubſtance, in an apparently half charred ſtate, retaining diſtinctly the form of the leaves, and the arrangement of the fibres.

External characters.

The ſchiſtus is light, brittle, of eaſy oxfoliation, in the tranſverſe fracture earthy, and of a pale brown colour; but, when longitudinally divided, the whole ſurface conſtantly preſents a ſeries of the leaves which have been mentioned, uniformly ſpread, and commonly of a light gray on the upper ſurface, and of a dark brown on the other; the fibres on the

* Strata of bituminous wood are found in various parts of France, in the vicinity of Cologne, in Heſſe, Bohemia, Saxony, Italy, and eſpecially in Iceland, where it is known under the name of Surturbrand.

light

light gray ſurface being generally of a blackiſh-brown, which is alſo the colour aſſumed by the ſchiſtus when reduced to powder.

The leaves appeared to be in the ſtate of charcoal, by being extremely brittle, by the blackiſh brown colour, by deflagrating with nitre, by the manner of burning, and by forming carbonic acid. I was, however, ſoon convinced that the ſubſtance of theſe leaves was not complete charcoal, but might more properly be regarded as vegetable matter in an incipient ſtate of carbonization, which, although poſſeſſed of many of the apparent porperties of charcoal, ſtill retained a ſmall portion of ſome of the other principles of the original vegetable.

The leaves almoſt in the ſtate of coal;

My ſuſpicion was excited, partly by the odour produced during combuſtion, which rather more reſembled that of wood than that of charcoal, and partly by the brown ſolution formed by digeſting the powder of the unburned ſchiſtus in boiling diſtilled water; for, by various teſts I aſcertained, that the ſubſtance thus diſſolved was not of a mineral nature. In order, however, fully to ſatisfy myſelf in this reſpect, I digeſted 250 grains of the pulverized ſchiſtus with ſix ounces of water.

but burning with the odour of wood.

Digeſtion of the ſchiſtus in water, and examination of the ſolution.

The liquor was, as before, of a dark brown colour.

It had but little flavour.

Pruſſiate of potaſh, muriate of barytes, and ſolution of iſinglaſs, did not produce any effect; nitrate of ſilver formed a very faint cloud; ſulphate of iron was ſlowly precipitated, of a dark browniſh colour; and muriate of tin produced a white precipitate.

A portion of the ſolution, by long expoſure to the air, was partially decompoſed; and a quantity of a brown ſubſtance was depoſited, which could not again be diſſolved in water.

Another portion was alſo evaporated to dryneſs, and afforded a ſimilar brown ſubſtance, which was only partially ſoluble in water; and the reſiduum, in both of the above caſes, was found to be inſoluble in alcohol, and in ether.

When burned, it emitted ſmoke, with the odour of vegetable matter.

250 grains of the ſchiſtus, afforded about three grains of the above ſubſtance; and, when the properties of the aqueous ſolution are conſidered, ſuch as its partial decompoſition, and the depoſit which it yielded by expoſure to air, and by evaporation;

tion; the inſolubility of this depoſit when again digeſted with water, alcohol, or ether; the ſmoke and odour which it yielded when burned; and the precipitates formed by the addition of ſulphate of iron and muriate of tin to its ſolution; when theſe properties, I ſay, are conſidered, there ſeems much reaſon to conclude, that the ſubſtance diſſolved by water was vegetable extract, which had apparently ſuffered ſome degree of modification, but not ſufficient to annul the more prominent characteriſtic properties of that ſubſtance.

The powder of the ſchiſtus, which had been employed in the preceding experiment, was afterwards digeſted in alcohol during two days; and a pale yellow tincture was thus formed, which, by evaporation, left about one grain of a yellow tranſparent ſubſtance, poſſeſſing the properties of reſin.

It contained vegetable extract, and a little reſin,

but no tannin.

It appears, therefore, that a ſubſtance very analogous to vegetable extract, and a ſmall portion of reſin, remain inherent in the leaves of this remarkable ſchiſtus.

As ſolution of iſinglaſs did not produce any effect, there was reaſon to conclude, that the aqueous ſolution above-mentioned did not contain any tannin; but, as the tannin might be combined with the alumina of the ſchiſtus, I digeſted a portion of it in muriatic acid, which, after filtration, was evaporated almoſt to dryneſs, leaving, however, the acid in a ſlight exceſs. This was diluted with water; and afforded a blue precipitate with pruſſiate of potaſh, a yellowiſh precipitate with ammonia, and a white precipitate with muriate of tin, but not any with ſolution of iſinglaſs. The tannin which might have been contained in the recent vegetable, appears therefore to have been diſſipated or decompoſed, with the greater part of the other vegetable principles, excepting the woody fibre reduced to the ſtate of an imperfect coal, and the ſmall portions of extract and reſin which have been mentioned.

Previous to having made the analyſis, I had an idea, that this ſchiſtus might be a lamellated incruſtation, formed by the tufa of the hot ſprings; but, according to Mr. Klaproth's analyſis *, the tufa of Geyſer is compoſed of,

Silica - -	98
Alumina - - -	1.50
Iron - -	50
	100.

* *Beiträge*; *Zweiter band*, p. 109.

It

It is therefore very different from the ſchiſtus, the component ingredients of which were aſcertained from the following analyſis.

ANALYSIS OF THE SCHISTUS FROM ICELAND*.

Analyſis of the ſchiſtus.

A. 250 grains, by diſtillation, yielded water, which, in the latter part of the proceſs, became ſlightly acid and turbid, =42.50 grains.

B. The heat was gradually increaſed, until the bulb of the retort was completely red hot. During the increaſe of the heat, a thick brown oily bitumen came over, which weighed 7.50 grains; it was attended with a copious production of hydrogen, carbonated hydrogen, and carbonic acid, the whole of which may be eſtimated at 23.75 grains.

C. The reſiduum was black, like charcoal, and weighed 176.25 grains; but, being expoſed to a ſtrong red heat in a crucible of platina, it burned with a faint lambent flame, and was at length reduced to a pale brown earthy powder, which weighed 122 grains; ſo that 54.25 grains were conſumed.

D. The 122 grains were mixed with 240 of pure potaſh; and, as ſome particles of charcoal remained, 50 grains of nitre were added, and the whole was ſtrongly heated, during half an hour, in a ſilver crucible. The maſs was then diſſolved in diſtilled water, and, muriatic acid being added to exceſs, the liquor was evaporated to dryneſs, and was again digeſted with muriatic acid much diluted; a quantity of pure ſilica then remained, which, after having been expoſed to a red heat, weighed 98 grains.

E. The liquor from which the ſilica had been ſeparated, was evaporated nearly to dryneſs, and added to boiling lixivium of potaſh; after the boiling had been continued for about one hour, the liquor was filtrated, and a quantity of oxide of iron was collected, which amounted to 6 grains.

F. Solution of muriate of ammonia was added to the preceding filtrated liquor; and, the whole being then heated, a copious precipitate of alumina was obtained, which, after having been made red-hot, weighed 15 grains.

Carbonate of ſoda cauſed the preceding liquor (after the ſeparation of alumina) to become ſlightly turbid, but not any precipitate could be collected.

* The remaining ſpecimens are now in the Britiſh Muſeum, and in the collection of the Right Hon. Charles Greville.

By

Component parts.

By this analyſis, 250 grains of the ſchiſtus afforded,

		Grains.
Water - - -	A. -	42.50
Thick brown oily bitumen	B. -	7.50
Mixed gas (by computation)		23.75
Charcoal (by computation)	C. -	54.25
Silica - - - -	D. -	98
Oxide of iron - -	E. -	6
Alumina - - - -	F. -	15
		247.

But the water and vegetable matter muſt be regarded as extraneous; and, if they are deducted, the real compoſition of the ſchiſtus is nearly as follows.

Its component parts, excluſive of water and vegetable matter.

Silica - - - -	82.30
Alumina - - - -	12.61
Oxide of Iron - - -	5.
	99.91.

It evidently, therefore, belongs to the family of argillaceous ſchiſtus, although the proportion of ſilica is more conſiderable than has been found in thoſe hitherto ſubjected to chemical analyſis.

The ſchiſtus has not been noticed by von Troil, nor by any of thoſe who have written concerning Iceland; for the ſlate which was ſent to Profeſſor Bergmann by the former, and which is mentioned by the latter in one of his letters, is there expreſsly ſtated to be the common aluminous ſlate containing impreſſions *.

§ IV.

Queſtion, whether the circumſtances of the production of this ſchiſtus were peculiar.

From the experiments which have been related, we find that the leaves contained in the Iceland ſchiſtus, although they are apparently

* Letters on Iceland, by Uno von Troil, p. 355.

Mr. Faujas St. Fond has however deſcribed a ſchiſtus nearly ſimilar, which is found near Roche-Seauve, in the Vivarais. The ſtratum extends about two leagues; and the only difference is, that, according to Mr. St. Fond, the ſchiſtus at Roche-Seauve is of the nature of marble, or, as he terms it, argillo-calcareous, whereas this of Iceland is ubdoubtedly argillaceous. From Mr. St. Fond's account, it does not appear that the vegetable leaves contained in the ſchiſtus of Roche-Seauve have been chemically examined. *Eſſai de Geologie*, par. B. Faujas St. Fond, Tome I. pp. 128 and 134.

apparently reduced almoſt to the ſtate of charcoal, nevertheleſs retain ſome part of their original proximate principles, namely, extract and reſin. This, of itſelf, is undoubtedly a remarkable fact; but, if it were unſupported by any other, the only inference would be, that the ſchiſtus was moſt probably of very recent formation, and had been produced under peculiar circumſtances.

requires an attention to other mineral bitumens.

I was deſirous, therefore, to diſcover ſome ſimilar caſes, which might ſerve as additional corroborative proofs of the gradual alterations by which vegetable bodies become changed, ſo as at length to be regarded as forming part of the mineral kingdom; and, from the reaſons which have been ſtated in the commencement of this paper, as well as from a certain ſimilarity in the external characters of the ſubſtance compoſing the leaves above-mentioned with thoſe of the Bovey coal, I was induced to make this laſt alſo a ſubject of chemical inquiry.

Bovey coal.

In the Philoſophical Tranſactions for the year 1760*, ſome remarks on the Bovey coal, and an account of the ſtrata, are ſtated, in a letter from the Rev. Dr. Milles to the Earl of Macclesfield. The object, indeed, of the author, was to eſtabliſh that this and ſimilar ſubſtances are not of vegetable, but of mineral origin; and, to prove this, he adduces a great number of caſes, moſt of which, however, in the preſent ſtate of natural hiſtory and of chemiſtry, muſt be regarded as proving the contrary; whilſt others, mentioned by him, ſuch as the Kimmeridge or Kimendge coal, are nothing more than bituminous ſlates, and of courſe are of a very different nature.

Its hiſtory.

Dr. Milles's account of the varieties of the Bovey coal, and of the ſtate of the pits at that time, appears to be very accurate; and, for the preſent ſtate, or at leaſt ſuch as it was in 1796, I ſhall beg leave to refer to a paper of mine, publiſhed in the fourth volume of the Tranſactions of the Linnean Society †; for, as this is more immediately a chemical inveſtigation, I wiſh to avoid, as much as poſſible, entering into any minute detail of geological circumſtances.

* Vol. LI. p. 534.

† Obſervations on bituminous Subſtances, p. 138. See alſo Parkinſon's Organic Remains of a former World. Vol. I. p. 126.

It

Its ſtrata agree with thoſe of ſurturbrand.

It may however be proper to obſerve, that the Bovey coal is found in ſtrata, correſponding in almoſt every particular with thoſe of the ſurturbrand in Iceland, deſcribed by Von Troil *, and by Profeſſor Bergmann †. The different ſtrata of both theſe ſubſtances are likewiſe ſimilar, being compoſed of wood or trunks of trees, which have completely loſt their cylindrical form, and are perfectly flattened, as if they had been ſubjected to an immenſe degree of preſſure ‡.

The

* Von Troil's Letters, p. 42.

† *Opuſcula Bergmanni*, Tom. III. *De Productis Volcaniis*, p. 239.

‡ Bergmann, in the diſſertation above quoted, accurately deſcribes this appearance of the ſurturbrand, and then ſays, "Quæ autem immanis requiritur vis, ut truncus cylindricus ita complanetur? *Nonne antea particularum nexus putredinis quodam gradu fuerit relaxatus?* Certe, niſi compages quodammodo mutatur, quodlibet pondus incumbens huic effectui erit impar. Ceterùm idem obſervatur phænomenon *in omni ſchiſto argillaceo.*" This is certainly a very curious fact; and the learned Profeſſor, with his uſual acuteneſs, rejects the idea that mere weight can have been the cauſe. As a farther proof alſo, he afterwards obſerves, "Orthoceratitæ, quæ *in ſtrato calcareo conicam figuram perfectè ſervant*, in ſchiſto *planum fere triangulare compreſſione efficiunt.* Idem valet de piſcibus, conchis, inſectiſque petrefactis." And again, "Obſervatu quoque dignum eſt, quod *idem reperiatur effectus, quamvis ſtratum calcareum ſub ſchiſto collocatum ſit, et majori ideo pondere comprimente onuſtum.*" *De Productis Volcaniis*, p. 240. It is evident, therefore, that weight alone has not produced this effect; and Bergmann's idea, that the ſolidity of the vegetable bodies may have undergone ſome previous change, in the manner of incipient putrefaction, by moiſture, and by becoming heated in the maſs, muſt be allowed to be very probable. But bodies ſuch as ſhells could not be thus affected; and therefore they muſt have been expoſed to ſome mechanical effect, peculiar to argillaceous ſtrata; which effect, however, from the circumſtances which have been adduced, evidently could not have reſulted from the mere preſſure of the ſuperincumbent ſtrata. To me, therefore, it ſeems not very improbable, that, together with a certain change in the ſolidity of vegetable bodies, produced in the manner imagined by Bergmann, and, together with ſome degree of ſuperincumbent preſſure, a real and powerful mechanical action has been exerted, by the contraction of the argillaceous ſtrata, in conſequence

of

The Bovey coal is commonly of a chocolate-brown, and ſometimes almoſt black. The quality and texture of it are various in different ſtrata; from ſome of theſe, it is obtained in the form of ſtraight flat-pieces, three or four feet in length, reſembling boards, and is therefore called Board Coal. Others have an oblique, wavy, and undulating texture, and, as Dr. Milles obſerves, have a ſtrong reſemblance to the roots of trees, from which, moſt probably, this ſort has in a great meaſure been formed. External characters of Bovey coal.

Some kinds alſo appear to be more or leſs intermixed with earth; but that which produces the moſt powerful and laſting fire, is called ſtone coal; it is black, with a gloſſy fracture; has little or none of the vegetable texture; is more ſolid and compact than the others, being almoſt as heavy as ſome of the pit coals, the nature of which it ſeems very nearly to approach.

For chemical examination, I ſelected ſome of the coal which had a wavy texture, and rather a gloſſy fracture; the quality of this ſort being apparently intermediate between the others, as it retains completely the marks of its vegetable origin, while, at the ſame time, it poſſeſſes every perfect character of this ſpecies of coal.

A. 200 grains of the Bovey coal, by diſtillation, yielded, Chemical examination of Bovey coal.

	Grains.
1. Water, which ſoon came over acid, and afterwards turbid, by the mixture of ſome bitumen - -	60
2. Thick brown oily bitumen - - - -	21
3. Charcoal - - - - - - - -	90
4. Mixed gas, conſiſting of hydrogen, carbonated hydrogen, and carbonic acid, } eſtimated at	29
	200.

The charcoal, in appearance, perfectly reſembled that which is made from recent vegetables. By incineration, about 4 grains of yellowiſh aſhes were left, which conſiſted of alumina, iron, and ſilica, derived moſt probably from ſome ſmall portion of the clay ſtrata which accompany the Bovey coal.

of deſiccation; this, I believe, has not hitherto been much conſidered, but I am inclined, from many circumſtances, to attribute to it a very great degree of power.

But

But it is very remarkable, that neither the aſhes obtained from the charcoal of the Bovey coal, nor thoſe obtained from the leaves of the Iceland ſchiſtus, afforded the ſmalleſt trace of alkali.*

B. 200 grains of the Bovey coal, reduced to powder, were digeſted in boiling diſtilled water, which was afterwards filtrated, and examined; but I could not diſcover any ſigns of extract, or of any other ſubſtance.

C. 200 grains were next digeſted with ſix ounces of alcohol, in a very low degree of heat, during five days. A yellowiſh-brown tincture was thus formed, which, by evaporation, afforded a deep brown ſubſtance, poſſeſſing all the properties of reſin, being inſoluble in water, but ſoluble in alcohol, and in ether; it alſo ſpeedily melted, when placed on a red-hot iron, burned with much flame, and emitted a fragant odour, totally unlike the very unpleaſant ſmell produced by burning the coal itſelf, or by burning any of the common bituminous ſubſtances. The quantity, however, which could be extracted from 200 grains of the coal, by alcohol, was but ſmall, as it did not exceed 3 grains. But this ſmall quantity was ſufficient to prove, that although the Bovey coal does not contain any vegetable extract, like the ſchiſtus formerly mentioned, yet the whole of the proximate principles of the original vegetable have not been entirely changed; as a ſmall portion of true reſin, not converted into bitumen, ſtill remains inherent in the coal, although the bituminous part is much the moſt prevalent, and cauſes the fetid odour which attends the combuſtion of this ſubſtance.

It reſembles the ſchiſtus in its products as well as character; but it afforded no extract, but only reſin;

Upon a compariſon of the general external characters of the Bovey coal, with thoſe of the ſubſtance which forms the leaves contained in the Iceland ſchiſtus, a very great reſemblance will be obſerved; and this is farther confirmed, by the ſimilarity of the products obtained from each of them in the preceding experiments, with the ſingle exception, that the leaves contain

* This, as far as relates to the Bovey coal, has been alſo noticed by Dr. Milles, Phil. Tranſ. Vol. LI. p. 553. But wood, however long ſubmerged, is not deprived of alkali, unleſs it has more or leſs been converted into coal; for I have, ſince the reading of this paper, made ſome experiments on the wood of the ſubmerged foreſt at Sutton, on the coaſt of Lincolnſhire, and have found it to contain potaſh.

ſome

ſome vegetable extract, which I could not diſcover in the Bovey coal. They agree however in every other reſpect; as they both conſiſt of woody fibre in a ſtate of ſemicarbonization, impregnated with bitumen, and a ſmall portion of reſin, perfectly ſimilar to that which is contained in many recent vegetable bodies; and thus it ſeems, that as the woody fibre, in theſe caſes, ſtill retains ſome part of its vegetable characters, and is but partially and imperfectly converted into coal, ſo, in like manner, ſome of the other vegetable principles have only ſuffered a partial change. Undoubtedly, there is every reaſon to believe that, next to the woody fibre, reſin is the ſubſtance which in vegetables paſſing to the foſſil ſtate, moſt powerfully reſiſts any alteration; and that, when this is at length effected, it is more immediately the ſubſtance from which bitumen is produced. The inſtances which have been mentioned corroborate this opinion; for the vegetable extract in one of them, and more eſpecially the reſin which was diſcovered in both, muſt be regarded as part of thoſe principles of the original vegetables which have remained, after ſome other portions of the ſame have been modified into bitumen.

which moſt probably is that which affords the bitumen.

The ſmallneſs of the quantity of reſin obtained in both the preceding caſes, by no means invalidates the proof of the above opinion; but, as an additional confirmation of it, I ſhall now give an account of a very ſingular ſubſtance, which is found with the Bovey coal.

§ V.

Dr. Milles, in his remarks on the Bovey coal, (which I have ſeveral times had occaſion to notice in the courſe of this Paper,) ſtates, that "amongſt the clay, but adhering to the coal, are found lumps of a bright yellow *loam*, extremely light, and ſo ſaturated with petroleum, that they burn like ſealing wax, emitting a very agreeable and aromatic ſcent." *

Singular ſubſtance found with the Bovey coal.

This ſubſtance, I alſo obſerved, when I viſited the Bovey coal-pits, in 1794 and 1796. At that time, however, it was ſcarce, and I could only procure one ſmall ſpecimen, which is now in the Britiſh Muſeum; but, from a curſory examination of it, I was convinced that it was a peculiar bituminous ſubſtance,

It is a peculiar bitumen.

* Phil. Tranſ. Vol. LI. p. 536.

and not loam impregnated with petroleum, as Dr. Milles had ſuppoſed. I could not then conveniently make a regular analyſis of it, and therefore contented myſelf with briefly deſcribing it, in a note annexed to my Paper on bituminous Subſtances. *

Lately, however, my friend John Sheldon, Eſq. of Exeter, F. R. S. obligingly ſent me ſeveral pieces of it, together with ſpecimens of the different kinds of Bovey coal which have been mentioned; and thus I was enabled fully to aſcertain its real nature and properties.

DESCRIPTION OF THE BITUMEN FROM BOVEY.

Deſcription of the bitumen from Bovey.

It accompanies the Bovey coal, in the manner already deſcribed, and is found in maſſes of a moderate ſize.

The colour is pale browniſh ochraceous yellow.

The fracture is imperfectly conchoidal.

It appears earthly externally, but, when broken, exhibits a ſlight degree of vitreous luſtre.

The fragments are irregularly angular, and completely opaque at the edges.

It is extremely brittle.

It does not apparently become ſoftened, when held for ſome time in the hand, but emits a faint reſinous odour.

The ſpecific gravity, at temperature 65° of Fahrenheit, is, 1,135.

Some ſpecimens have dark ſpots, ſlightly approaching in colour and luſtre to aſphaltum; and ſmall portions of the Bovey coal are commonly interſperſed in the larger maſſes of this bitumen.

When placed on a heated iron, it immediately melts, ſmokes much, burns with a bright flame, and yields a very fragrant odour, like ſome of the ſweet-ſcented reſins, but which at laſt becomes ſlightly tainted with that of aſphaltum.

The melted maſs, when cold, is black, very brittle, and breaks with a gloſſy fracture.

EXPERIMENTS.

Experiments on the ſame.

A. 100 grains of this bitumen, when diſtilled until the bulb of the retort became red-hot, afforded,

* Tranſactions of the Linnean Society, Vol. IV. p. 139.

	Grains.
1. Water ſlightly acid	3
2. Thick brown oily bitumen, very ſimilar to that which was obtained from the Bovey coal, but poſſeſſing ſlightly the odour of vegetable tar	45
3. Light ſpongy coal	23
4. Mixed gas, compoſed of hydrogen, carbonated hydrogen, and carbonic acid, (by computation,)	29.

The coal yielded about three grains and a half of aſhes, which conſiſted of alumina, iron, and ſilica, with a trace of lime.

B. The bitumen was not affected by being long digeſted in boiling diſtilled water.

C. By digeſting 100 grains in lixivium of pure potaſh, a brown ſolution was formed; this was ſaturated with muriatic acid, and a brown reſinous precipitate was obtained, which weighed 21 grains.

D. A portion was digeſted in nitric acid: at firſt, much nitrous gas was evolved, and, after the digeſtion had been continued for nearly 48 hours, a part was diſſolved, and formed an orange-coloured ſolution, which did not yield any precipitate, when ſaturated by the alkalis, or by lime; the colour only became more deep, and, by evaporation, a yellow viſcid ſubſtance was obtained, which was ſoluble in water. The above nitric ſolution poſſeſſed every property of thoſe nitric ſolutions of reſinous ſubſtances which I have mentioned in a former paper. *

E. The benzoic and ſuccinic acids were not obtained from this ſubſtance, by any of the methods uſually employed.

F. Alcohol almoſt immediately began to act upon this bitumen; and, being added at different times, gradually diſſolved a conſiderable part of it. The ſolution was reddiſh-brown, and has a reſinous odour; by the addition of water it became milky, and, by evaporation, afforded a dark brown ſubſtance, which had every property of reſin, whilſt the reſiduum left by the alcohol poſſeſſed thoſe proporties which characterize aſphaltum.

The following analyſis was then made, to diſcover the proportions of the component ingredients.

* Phil. Tranſ. for 1804, p. 198.

ANALYSIS OF THE BITUMEN FROM BOVEY.

Analyſis of the bitumen from Bovey.

A. 100 grains, reduced to a fine powder, were digeſted during 48 hours with ſix ounces of alcohol, the veſſel being placed in ſand moderately warmed. A deep reddiſh-brown tincture was thus obtained; and the operation was again twice repeated, with other portions of the ſame menſtruum, until it ceaſed to act upon the reſiduum.

The whole of the ſpirituous ſolution (which had been cautiouſly decanted) was then ſubjected to a very gradual diſtillation in an alembic, and yielded a brown fragant reſin, which weighed 55 grains.

B. The reſiduum, which could not be diſſolved by alcohol, was digeſted in boiling diſtilled water, but this did not act upon it; the whole was therefore collected on a filter, was gradually dried, without heat, by mere expoſure to the air, and then weighed 44 grains.

Theſe 44 grains conſiſted of a light, porous, pale-brown ſubſtance, which, being melted, formed a black, ſhining, brittle maſs. It burned with the odour of aſphaltum, but rather leſs diſagreeable, owing moſt probably to a ſmall portion of the reſin, which had not been completely extracted by the alcohol. It was inſoluble in water, and in alcohol, but was readily diſſolved by heated fat oils; and in every other particular was found to poſſeſs the properties of aſphaltum.

The 44 grains of aſphaltum, when burned, left a reſiduum, which weighed three grains, and conſiſted of alumina, ſilica, and iron.

By this analyſis it appears, that the bitumen which accompanies the Bovey coal, is a peculiar and hitherto unknown ſubſtance, which is partly in the ſtate of vegetable reſin, and partly in that of the bitumen called aſphaltum, the reſin being in the largeſt proportion, as 100 grains of the above-mentioned ſubſtance afforded,

Its component parts.

Reſin - - - - -	55
Aſphaltum - - - -	41
Earthy reſiduum - - -	3
	99.

Thus we have an inſtance of a ſubſtance being found under circumſtances which conſtitute a foſſil, although the characters of it appertain partly to the vegetable, and partly to the mineral kingdom.

§ VI.

§ VI.

The powerful action which alcohol exerts on moſt of the reſins, may juſtly be regarded as forming a marked diſtinction between thoſe ſubſtances and the bitumens. But, as ſome of the bitumens are acted upon by alcohol, in a ſlight degree, I was deſirous to aſcertain whether a ſmall portion of reſin was contained in any of theſe; or, if that was not the caſe, I wiſhed to determine the nature of the ſubſtances which could be ſeparated, although very ſparingly, by this menſtruum. I therefore made the following comparative experiments, on the ſoft brown elaſtic bitumen from Derbyſhire; on the genuine aſphaltum; on very pure cannel coal; and on the common pit coal. Experiments on bitumens by the action of alcohol.

100 grains of each were digeſted with three ounces of alcohol, in matraſſes placed in warm ſand, during five days, ſome alcohol being occaſionally added, to ſupply the loſs cauſed by evaporation. After the above-mentioned period had elapſed, the liquid contained in each matraſs was poured into ſeparate veſſels.

I. The alcohol which had been digeſted on the elaſtic bitumen was not tinged, nor, when ſpontaneouſly evaporated, did it leave any film or ſtain on the glaſs. Reſults.

II. From aſphaltum, the alcohol had extracted a yellow tincture, which, in ſome ſituations, appeared of a pale olive colour, and, being ſpontaneouſly evaporated, a thick brown liquid was depoſited, in ſmall drops, on the glaſs; theſe drops did not become hard after two months, and poſſeſſed the odour, and every other property, of petroleum. The aſphaltum had loſt in weight about one grain and a half.

III. The cannel coal had communicated a pale yellow tint to the alcohol, which, in the manner above deſcribed, was aſcertained to be cauſed by petroleum; but, from the ſmallneſs of the quantity, the weight could not be determined.

IV. The alcohol which had been digeſted on pit coal, had not aſſumed any colour; but, by ſpontaneous evaporation *, it left a film on the glaſs, which, by its odour, was alſo found to be petroleum.

By theſe experiments we find, that the action of alcohol on the bitumens is very ſlight; and that the ſmall portion which Alcohol acts ſlightly on bitumens.

* Spontaneous evaporation, by expoſure to the air, was employed in theſe experiments, for reaſons which muſt be ſufficiently obvious.

may

may thus be extracted from ſome of them, is petroleum. In theſe, the proceſs of bituminization (if I may be allowed to employ ſuch a term) appears to have been completed, whilſt in the Bovey coal, and eſpecially in the ſubſtance which accompanies it, nature ſeems to have performed only the half of her work, and, from ſome unknown cauſe, to have ſtopped in the middle of her operations. But, by this circumſtance, much light is thrown on the hiſtory of bituminous ſubſtances; and the opinion, that they owe their origin to the organized kingdoms of nature, eſpecially to that of vegetables, which hitherto has been ſupported only by preſumptive proofs, ſeems now, in a great meaſure, to be confirmed, although the cauſes which operate theſe changes on vegetable bodies are as yet undiſcovered.

They are not formed by time alone.

Many facts indicate, that time alone does not reduce animal or vegetable bodies to the ſtate of foſſils. In this country, there are numerous examples of large quantities of timber, (even whole foreſts) which have been ſubmerged prior to any tradition, and which nevertheleſs completely retain their ligneous characters.* Other local cauſes and agents muſt therefore have been required, to form the varieties of coal and other bituminous ſubſtances. In ſome inſtances (as in the formation of Bovey coal), theſe cauſes ſeem to have acted partially and imperfectly, whilſt, in the formation of the greater part of the pit coals, their operation has been extenſive and complete.

In the pit coals, the mineral characters predominate, and the principal veſtige of their real origin ſeems to be bitumen; for the preſence of carbon in the ſtate of oxide, cannot alone be conſidered as deciſive.

Bitumen, therefore, with the exuviæ and impreſſions ſo commonly found in the accompanying ſtrata, muſt be more immediately regarded as the proofs, in favour of the origin of pit coal from organized bodies; and, conſidering the general facts which have been long obſerved, together with thoſe lately adduced reſpecting the Bovey coal, and the ſubſtance which is found with it, we ſeem now to have almoſt unqueſ-

* Phil. Tranſ. for January, 1671. Phil. Tranſ. Vol. XIX. p. 526. Ibid. Vol. XXII. p. 880. Ibid. Vol. XXIII. p. 1073. Ibid. Vol. XXVII. p. 298. Ibid. for 1799, p. 145.

tionable

tionable evidence, that bitumen has eſſentially been produced by the modification of ſome of the proximate principles of vegetables, and eſpecially reſin.

Whether they be of igneous or aqueous formation.

Modern chemiſtry had comparatively made but a ſmall progreſs, when the illuſtrious Bergmann publiſhed his Diſſertation, entitled, *Producta Ignis ſubterranei chemice conſiderata;* for, at that time, the extent and power of chemical action, in the humid way, were very imperfectly underſtood. In that part, however, of the above work, where he ſpeaks of the foſſil wood of Iceland, called Surturbrand, he evidently appears doubtful how far volcanic fire may have acted upon it; although he conceives that, in the formation of it there has been ſome connection with volcanic operations. His words are, "*Quid de ligno foſſili Iſlandiæ ſentiendum ſit, gnaro in loco natali contemplatori* decidendum relinquimus. *Interea, ut cum vulcani operationibus nexum credamus, plures ſuadent rationes, quamvis hucuſque modum ignoremus, quo ſitum texturamque adquiſiverunt hæc ſtrata.*" It certainly was very natural that Bergmann ſhould entertain this opinion, in reſpect to the ſurturbrand; and it is remarkable, that the leaves contained in the ſchiſtus lately deſcribed, are of the ſame nature, and are found in the ſame country. The leaves alſo deſcribed by Mr. St. Fond, are likewiſe found in a country which, according to him, was formerly volcanic. Were theſe ſubſtances, therefore, never found but in countries which either actually are or were volcanic, we ſhould be almoſt compelled to believe, with the Swediſh profeſſor, that the operations of ſubterraneous fires have been concerned in the formation of theſe bodies, or rather in the converſion of them into their preſent ſtate.

They are found in countries evidently not volcanic.

But ſimilar ſubſtances are found in countries where not the ſmalleſt veſtige of volcanic effects can be diſcovered, and Devonſhire moſt undoubtedly is ſuch; yet, nevertheleſs, the Bovey coal is there found ſimilar to the ſurturbrand, in moſt of the external, and (from experiments which I made ſome years ago, I believe I may ſay) chemical properties; to which muſt be added, that both theſe ſubſtances perfectly reſemble each other, by forming regular ſtrata.*

* Tranſ. of the Linnean Society, Vol. IV. p. 138. Von Troil's Letters, p. 42. *Opuſcula Bergmanni*, Tom. III. p. 239.

Moreover,

Deduction.

Moreover, the half charred appearance of Bovey coal, and of ſurturbrand, cannot be adduced as any proof, that the original vegetable bodies have been expoſed to the partial effects of ſubterraneous fire; for, at this time, we know that the oxidizement of ſubſtances is performed, at leaſt as frequently, and as effectually, by the humid as by the dry way. It would therefore be ſuperfluous here, to enter into an elaborate diſcuſſion, to prove that coal and bitumen, with much greater probability, have been formed without the intervention of fire; and I am the leſs inclined to ſay more upon this ſubject, as I have already publiſhed ſome conſiderations on it in a former paper.*

The new ſubſtance found with the Bovey coal termed *Retinaſphaltum*.

Before I conclude, I muſt beg leave to obſerve, that as the ſubſtance which is found with the Bovey coal is, in every reſpect, ſo totally different from any of the bitumens hitherto diſcovered, it ſeems proper that it ſhould receive ſome ſpecific name; and, as it has been proved to conſiſt partly of a reſin and partly of a bituminous ſubſtance, I am induced to call it *Retinaſphaltum*,† a name by which a full definition of its nature is conveyed.

Account of another ſpecimen, probably of the ſame.

I have lately ſeen, in No. 85 of the *Journal des Mines*, p. 77, an account of a peculiar combuſtible foſſil, found near Helbra, in the county of Mansfield, and deſcribed by Mr. Voight, in his *Verſuch einer Geſchichte der Steinkohle, der Brawnkohle*, &c. p. 188. This ſubſtance is of an aſh-coloured gray, paſſing to grayiſh-white; it is found in a bed of bituminous vegetable earth, which has apparently been produced by the decompoſition of foſſil wood. The pureſt ſpecimens are in the form of nodules; the fracture is earthy; it is opaque; ſoft; brittle; and is very light. When applied to the flame of a candle, it burns and melts like ſealing-wax, at the ſame time diffuſing an odour which is not diſagreeable. This ſubſtance appears to accord in ſo many properties with the retinaſphaltum of Bovey, that I cannot but ſuſpect it to be of a ſimilar nature, and I have little doubt that, by a chemical examination, it will be found to conſiſt partly of reſin and partly of bitumen.

* Tranſ. of the Linnean Society, Vol. IV. pp. 141, &c.

† From ῥητίνη, reſin; and ἄσφαλτος, bitumen.

VIII.

Account and Deſcription of an improved Air Pump. By Mr. N. MENDELSSOHN.

To Mr. NICHOLSON.

SIR,

Introductory letter.

ON entering into buſineſs, as a mathematical inſtrument-maker, I reſolved to make it my principal ſtudy to introduce into them all the improvements which the preſent advanced ſtate of ſcience, the nature of the ſubject, and my humble abilities would allow. Thus I have begun with improving *Volta's electrical lamp* and the *air-pump*. I imagine, I have rendered the conſtruction of the latter much more ſimple than it uſually is, and conſequently leſs liable to derangement, at the ſame time that it poſſeſſes a very great exhauſting power. Give me leave, Sir, to offer you a deſcription of this improved air-pump with the requiſite drawings. It is ready for your inſpection. I ſhall think myſelf very happy, if this uſeful philoſophical inſtrument meet with your approbation. And if it be ſo fortunate, I requeſt you the favour of introducing it into public notice, by inſerting my paper in your excellent and deſervedly celebrated Journal.

I am, with the higheſt eſteem, Sir,

Your humble and obedient ſervant,

N. MENDELSSOHN.

No. 50, *Surry-Street, Black-Friars,*
Feb. 13, 1805.

Deſcription of Mendelſſohn's improved air-pump.

Notwithſtanding the many improvements which have been made upon the conſtruction of the air-pump, it was ſtill deſirable that this inſtrument ſhould be more ſimplified in its mechaniſm, and yet poſſeſs the ſame advantages of thoſe conſtructed lately. That the mechanical power of the pump, and not the preſſure of the air, ſhould open the valves, has long been required and already done by Mr. Cuthbertſon, Mr. Haas, and ſeveral other ſkilful artiſts; but may I be allowed to remark, that on reading the deſcription of their inſtruments, they appeared to me to be too compounded. It muſt be very difficult

for

Deſcription of Mende'ſſohn's improved air-pump.

for an experimental philoſopher to clean an inſtrument which, being thus complicated, is not only rendered intricate, but is alſo difficult to be put together again. Being deſirous, therefore, to ſimplify this inſtrument, I adopted the conſtruction here deſcribed, by which it is capable of being put together in leſs than half an hour when cleaned, and requires that operation very ſeldom.

I ſubmit to the judgment of your ſcientific readers how far the preſent inſtrument anſwers its deſired purpoſe. I have rejected that tube which, in common air-pumps, leads from the valves to the receiver, together with the cock that ſerves to ſhut this pipe: the receiver is placed immediately upon the valves, theſe being put on the top of the cylinders, which, conſequently, required the rackwork and pinion to be underneath, and inverted the whole inſtrument. *See the adjoining drawing, Plate* VI. where A B and C D repreſent the two cylinders of glaſs ground and poliſhed inſide. E and F are the two valves that allow the cylinders to communicate with the receiver O through two very ſhort canals A B and C D (*Fig* 2, *Plate* VII) and the cock G. Two other valves that open into the atmoſphere are within the covers *i* and *k*, as may be ſeen in *Fig.* 1, where *e* repreſents one of them. M N is the receiver-plate of glaſs ground flat; P Q a barometer-gauge, upon the plan of the firſt Torricellian tube, as the eaſieſt to conſtruct and the moſt infallible in its effects. It will be found to be here quite out of the way, ſecure from being broke by accident, and the moſt in ſight. H K and I L are two braſs pillars that ſupport the whole. R S V W the uſual rackwork, having a double winch *l m*, which, upon trial, will be found preferable to a ſingle one.

It will now be neceſſary to ſhew how this pump acts, in which it will be ſufficient to explain the action of one cylinder, becauſe the other is in all parts alike. E is a conical metallic valve, from which a canal goes through the cock G up to the receiver, as is ſeen in *Fig.* 1 and 2, *Plate* VII. where all the parts are marked with the ſame letters. E T is a ſteel rod going through a leather box in the piſton U. The top of this rod is fixed to the valve E, and its bottom part ſlides in a ſmall hole with an allowance of 0,1 inch up and downward, conſequently the valve E can move no farther. When the piſton deſcends, it firſt opens the valve by puſhing the rod to the

the bottom of the hole. Then it ſlides down along the rod E T, and the air from the receiver has now free acceſs to the cylinder. When the piſton returns it lifts the rod E T, and thus ſhuts up the valve. Then the piſton ſlides again along the rod up to the top of the cylinder, condenſing the air above it, which air, by the leaſt condenſation, opens a valve *e*, *Fig.* 2, and eſcapes freely into the atmoſphere. This laſt valve has neither ſpring or additional weight to ſhut it, but ſhuts by its own weight (about a quarter of an ounce) as ſoon as the piſton is arrived to the top of the cylinder. Deſcriptiou of Mendelſſohn's improved air-pump.

The cylinders are made of glaſs, *and the piſtons of tin, ſo well fitted as to be air-tight, without the interpoſition of any leathers.* The friction of theſe two bodies is ſmall beyond expectation, a ſufficient proof that they will be durable. They poſſeſs the further advantage of being capable of ſtanding for even ſix months, after which time they will ſerve without being cleaned or repaired, becauſe they are not liable to be corroded by the oil which they contain, an inconvenience too general in braſs cylinders. After all, if the preſent pump ſhould want cleaning, it is an eaſy operation to take off the top piece *g h*, by unſcrewing the nuts H and I, when this piece, with all the apparatus upon it, will come off. Then each cylinder may very eaſily be ſlid off from the piſton, wiped out and replaced, after having greaſed its inſide with a little of the cleaneſt ſweet oil: The top is then to be put again in its place, and the two nuts H and I being ſcrewed upon it, the inſtrument is ready. Neither racks or pinion need to be taken out of their places, the cylinders ſtanding above them.

The cock is conſtructed ſo, that, being in the ſituation repreſented in *Fig.* 1, the communication is open between the cylinders, the receiver, and the barometer-gauge, and, by a quarter of a revolution, the cylinders are excluded, the receiver and gauge being ſtill left in communication. A little ſtopper in *Fig.* 3, ground into the cock, being open, air is admitted to the receiver if required.

The receiver-plate is of glaſs ground flat, as was mentioned before: this will be found preferable to braſs, becauſe cleaner, and never corroded by acids or water; it will beſides often prove very convenient in making experiments on electricity in the vacuum.

The

The whole inſtrument is fixed upon a mahogany table, which ſerves as a ſtand to it.

I will conclude by obſerving, that neither the employing of glaſs cylinders, or the method of opening the valves, are new, but, for aught I know, this is the firſt inſtrument of the kind ever executed in this country; as likewiſe the idea of putting the valves at top, and thus ſimplifying the inſtrument, ſeems to have eſcaped the attention of the eminent artiſts both here and abroad; as to my beſt knowledge, it has never been done or deſcribed any where. The metallic piſtons, without leathering, muſt certainly add to the durability, and diminiſh the great labour that uſually attends working an air-pump.

IX.

Letter concerning Palladium, from WILLIAM HYDE WOLLASTON, *M. D. F. R. S. the Diſcoverer of that Metallic Body.*

To Mr. NICHOLSON.

SIR,

The author adverts to former proceedings reſpecting palladium; of which he is the diſcoverer.

THE candour with which you communicated all circumſtances that came to your knowledge concerning palladium, at a time when the diſcoverer of that ſubſtance was yet unknown to you, demands my earlieſt acknowledgments, as having been the author of thoſe communications; and it is proper that I ſhould alſo expreſs the ſatisfaction I received on learning the reſpectable tribunal you nominated at my requeſt, for examining the merits of any attempts that might be made to form that ſubſtance artificially.

Reaſons why the diſcovery was firſtcommunicated to the world anonymouſly.

As I have already ſhewn (in a paper which you did me the honour to reprint in your Journal for January laſt, p. 34,) by what means a very ſmall quantity of Palladium may be extracted from the ore of platina, and as I have there examined the ſynthetic attempts to prove that this body was a compound, with a degree of attention which I thought due to the chemical ſkill of the perſon who propoſed them, as well as to the degree of uncertainty that muſt attend a ſubject entirely new; I cannot now adduce further *chemical* evidence, and can only add, for the information of thoſe whoſe judgment has been biaſſed by the difficulty of accounting for the production

duction of ſo large a quantity of palladium as was offered for ſale, that a proportional quantity of platina, from which the whole was extracted, was purchaſed by me a few years ſince, with the deſign of rendering it malleable for the different purpoſes to which it is adapted. That object has now been attained, and during the ſolution of it, various unforeſeen appearances occurred, ſome of which led me to the diſcovery of palladium; but there were other circumſtances which could not be accounted for by the exiſtence of that metal alone. On this, and other accounts, I endeavoured to reſerve to myſelf a deliberate examination of thoſe difficulties which the ſubſequent diſcovery of a ſecond new metal, that I have called rhodium, has ſince enabled me to explain, without being anticipated even by thoſe foreign chemiſts, whoſe attention has been particularly directed to this purſuit,

I remain, Sir,

Your obliged and obedient Servant,

W. H. WOLLASTON.

Feb. 23, 1805.

X.

Short Remark on Mr. Walker's laſt Letter reſpecting Focal Images. *By* C. L.

To Mr. NICHOLSON.

SIR,

I WOULD beg your indulgence for a very ſhort letter, in anſwer to Mr. Ezekiel Walker. The only reply which ſeems needful, on the ſubject of the temper in which he or I may have written, is that if his firſt paper had indicated more of the calm ſpirit of philoſophy, as well as of philoſophical correctneſs, my obſervations might either have been unneceſſary, or, perhaps, drawn up without any extraneous remark. As it is, I do not think myſelf entitled, nor am I indeed inclined to offer any ſtrictures upon his laſt; which I leave to the unbiaſſed judgment of your readers. On the preſent occaſion, I only wiſh to adhere to the experiments. Men of ſcience will find no difficulty in forming a proper eſtimate of our reaſonings. In plain language, therefore, I will beg leave

C. L. declines any farther diſcuſſion reſpecting Mr. Walker's experiments with lenſes; but denies the facts.

to

to wave any attention to the hiſtory of Mr. Walker's experiments, upon which I have animadverted, and to deny his facts. What may have deceived him in his proceedings, his admeaſurements, and his repeated regiſterings, is not for me to diſcuſs. If he will ſend his lenſes to you, and you ſhould find that, under any circumſtances whatever of diſtance or poſition, it be poſſible, by a mere alteration in the aperture, to produce a difference, as 2 to 3, in the length of the focal image, (ſee Philoſ. Journal, Vol. IX. p. 165.) I think it will be incumbent upon us to re-examine all the facts and demonſtrations, of what we have hitherto been in the habit of calling the ſcience of optics, in order to reconcile them with ſo ſtrange a reſult.

I am, Sir,
Your obliged
C. L.

XI.

A Communication on the Uſe of Green Vitriol, or Sulphate of Iron, as a Manure; and on the Efficacy of paring and burning depending, partly, on Oxide of Iron. By GEORGE PEARSON, M. D. *Honorary Member of the Board of Agriculture, F. R. S. From a Communication made by him to the Board, and inſerted in the fourth Volume of their Tranſactions.*

Sulphate of iron or martial vitriol hitherto ſuppoſed deſtructive of vegetation,

I TAKE leave to lay before this Honourable Board, an account of a ſubſtance as a manure, which I find, on examination, is one of the things, hitherto univerſally believed to be a poiſon to vegetables. Having aſcertained that this ſubſtance is what is commonly known by the name of vitriol of iron (the ſulphate of iron of the chemiſts), inveterate opinion prevented me for ſome time from accepting the teſtimony of it as a manure; but feeling the weight of the reſpectable evidence by whom it was atteſted, after conſideration I perceived that the fact in queſtion was not at variance with eſtabliſhed principles of vegetable philoſophy, as I ſhall, I think, make appear in this communication.

—but the contrary is true.

My friend John Williams Willaume, Eſq. of Tingrith in Bedfordſhire, having deſired his brother, Charles Dymoke Willaume, Eſq. to aſk my opinion of a ſaline ſubſtance collected

lected from peat, which has been used with profitable consequences as a manure in his neighbourhood; I proposed a set of queries to Mr. John W. Willaume, the answers to which, in the two following copied letters, comprehend the evidence I have to offer.

LETTER No. I.

To Dr. Pearson, from C. D. Willaume, Esq.

My dear Sir,

Letter of information concerning peat ashes or dust.

I received the inclosed last Saturday, and hope the answers to your queries will be satisfactory, and tend to elucidate this curious subject. Though the answers under the article *dust* only relate to your queries, yet my brother has thought proper to advert to the *ashes*, which you conceive to be a *caput mortuum;* but which have been used as, and have been supposed to be, a beneficial manure from time immemorial. I have reserved a piece of the peat from which the ashes are produced, and if you would wish to analyse it, I will send it you. Favour me with the result of your future inquiries on this subject, and I am,

My dear Sir, your's very sincerely,

C. D. Willaume.

Walham Green,
Aug. 24, 1801.

LETTER No. II.

From John W. Willaume, Esq. to C. W. Willaume, Esq.

Queries proposed by Dr. Pearson.

Queries respecting salt of peat.

1. How long has the salt of peat been used?
2. How much per acre is laid on?
3. On what kind of lands?
4. The effects of it on vegetation?
5. Whether it is mixed with dung manure, or lime?
6. In what parts of the country has it been employed?
7. Any other facts which can be collected relative to the use of this substance?

In answering the above queries, I shall divide the subject into three articles, 1st, the *peat* considered as an object of fuel; 2d, the *ashes*; 3d, the *salt of peat, or dust;* the two last as objects of manure.

1. *Peat*

Deſcription of peat.

1. *Peat.* The peat, which is found after the removal of the turf or exterior ſurface, to about a ſpade's depth, has long been known as an article of fuel. It is, however, uſed only by cottagers, who burn it on a brick hearth; it has been rejected from the parlour, the kitchen, the brewhouſe, &c. as being injurious to grates, and to all ſorts of veſſels put on it; it cannot be employed in the roaſting of meat, as it will impart a diſagreeable taſte, and it is deſtructive of all ſorts of furniture by the effluvia which it emits, or by the duſt or aſhes which may chance to be blown from it. If theſe diſagreeable conſequences could be obviated, it might be made an article of general conſumption as a ſubſtitute for coal, much to the advantage of the ſeller and conſumer; it is dug out in the form of a brick to a certain depth, well known to the common labourer. This depth muſt be carefully attended to, leſt you ſhould cut out the ſtaple, in which caſe it would never be retrieved; but, this circumſtance attended to, it will grow again to its former ſtate in the ſpace of fifteen years. Thus the whole moor is divided into proper portions, and periodically cut once in fifteen years.

Its aſhes.

2. *Aſhes.* The turf or ſurface, and ſuch parts of the peat as do not appear to be of the beſt quality, are laid up in conſiderable heaps, and reduced to aſhes by the action of fire. *The aſhes are red.*

Anſwer to Queries.

Anſwers to the queries as to the aſhes.

1. The aſhes have been long known as a manure, and the demand is on the increaſe.

2. The quantity uſually laid on an acre, by ſpreading or ſowing it, is fifty buſhels, either on graſs or arable land.

3. It is laid on hot land. By hot land, we underſtand ſandy, gravelly, chalky ſoils of a dry nature, ſuch as are burnt up on the long continuance of hot weather. It is moſt commonly uſed for graſſes; but is in conſiderable eſteem, as a manure, for oats or barley on land of the nature above-mentioned.

4. The vegetable effect is ſurpriſing, inaſmuch as it will double or treble a crop of any new ſown graſs, ſuch as trefoil, &c. I have ſeen the benefits ariſing from it on old paſture land much overgrown with moſs, which it effectually deſtroys, and produces in its ſtead white or Dutch clover. You may

trace

trace to an inch the ceſſation and recommencement of this manure. It is obſervable, that near the fire heaps, as far as the wind can carry the lighter parts of the aſhes, the production of clover is ſure to be abundant; it is equally favourable to the growth of barley or oats.

5. It is not mixed with lime, or any other manure.

6. Theſe aſhes are bought by a ſet of higlers, who carry them in bags loaded on aſſes to a conſiderable diſtance, where they are known to be in great repute; they muſt come exceſſively dear to the conſumer by this mode of conveyance. The farmers in the vicinity ſend for them in waggons, particularly Mr. Brumiger, near Sundon in Bedfordſhire, a conſiderable and intelligent farmer, who increaſes his conſumption every year, both for his graſs and arable land.

3. *The Salt of Peat, or Duſt.*

Anſwers reſpecting the ſalt of peat.

Anſwer to Queries.—1. The duſt or gray ſaline ſubſtance is produced by beating the earth containing this ſalt to a powder; it is found in particular ſpots, not univerſally, the earth not being equally impregnated with it in all places; it has not been known as a manure above ſix years; but on trial greatly increaſes in reputation and demand.

2. Fifty buſhels are the proper quantity per acre. This ſhould not be exceeded, for if it be laid on in too great abundance, it may prove extremely deleterious.

3. It is uſed for cold lands. By cold lands we underſtand clayey, or any wet grounds.

4. It will much improve the vegetation of ſowed graſſes, and old paſture, and is equally favourable to the production of corn; the ground, whether graſs or arable, being of a cold nature.

5. It is not mixed with lime, or any other ſubſtance.

6. The duſt is likewiſe bought by the higlers, and carried to great diſtances. The nearer farmers likewiſe ſend for the duſt in waggons, particularly Mr. Anſtie, of Dunſtable Houghton, and Mr. Smith, of Sundon, who hold this manure in great eſteem.

Your's, &c.

Tingrith, Aug. 19, 1801. J. W. Willaume.

Dr. Pearſon's Experiments, Obſervations, and Remarks on the Subſtance called Salt of Peat, or Duſt.

Dr. Pearſon's chemical examination of ſalt of peat or duſt.

1. It is a blackiſh gray, coarſe, and rather heavy powder. Has no ſmell; taſtes ſtrongly ſtyptic; readily diſſolves in the mouth; did not deliqueſce on expoſure to the air.

2. Diſſolves in four times its weight of water of the temperature of ſixty degrees of Fahrenheit, and in twice its weight of boiling hot water, giving a pale green coloured ſolution, with a trifling ſediment, which is inſoluble in muriatic acid.

3. To the ſolution (2.) I added a little liquid pruſſiate of vegetable alkali in a perfectly neutral ſtate, which occaſioned immediately a moſt abundant precipitation of pruſſiate of iron; and this teſt was added gradually, till no further precipitation took place.

4. Into the decanted and filtrated fluid (3) was poured liquid cauſtic volatile alkali, but without inducing any change.

5. Into the ſame fluid (3) was poured liquid carbonate of vegetable alkali, which produced a ſcarcely perceivable cloudy appearance.

6. Into the ſolution (3) was dropped the aqueous ſolution of muriate of baryt, which occaſioned immediately a milky appearance.

7. To the ſolution (3) I added the oxalic acid, and turbidneſs enſued.

8. A little of the powdery ſubſtance, called the ſalt of peat, with concentrated ſulphuric acid, produced no emiſſion of fumes, nor ſmell.

9. The ſolution (2) with muriate of baryt, immediately grew thick and white as cream.

10. The ſolution (2) with carbonate of potaſh, depoſited a very copious greeniſh ſediment; and the ſame effect enſued with cauſtic volatile alkali.

11. The ſolution (2) with oxalic acid, gave inſtantly a very turbid bluiſh green precipitation.

The preceding experiments manifeſted that the *peat ſalt* conſiſts of *ſulphate of iron*, vulgarly called green vitriol of iron, mixed with a very minute proportion of ſilicious earth, and of lime united either to ſulphuric acid, or to carbonic acid. But the preſence of the earths magneſia and argill; the uncombined

bined alkalies; the uncombined acids; are by theſe experiments excluded. In ſhort, the ſalt of peat is almoſt pure *ſulphate of iron.*

Remarks.

1. The ſalt of peat is, I apprehend, depoſited by evaporations which run over the moors, where it is found; and hence I ſhould expect many of ſuch waters to be ſtrongly impregnated with it, and in many parts the ſoil to be tinged red and yellow by ochre. Very likely * on enquiry much iron pyrites will be found on, or near the moors. Salt of peat how produced.

2. The quantity ſpread on land is ſaid to be fifty buſhels per acre, which I eſtimate at 2,250 pounds avoirdupoiſe; this will give near ſeven ounces and a half per ſquare yard. If a larger quantity be applied, it is obſerved it will prove extremely deleterious. This is true alſo of every other manure, ſuch as lime, alkaline ſalts, marine ſalt, nay, of the dung of animals: for if they be uſed in certain quantities, they *poiſon* plants, inſtead of promoting their growth. This is equally true in the animal kingdom; for there is not an article taken as food, or as ſeaſoning, which is not a poiſon, if taken in certain quantities. A human creature may be poiſoned or alimented by beef or pudding, according to the quantity of them taken into the ſtomach. He may be poiſoned, or have digeſtion greatly aſſiſted by ſalt, or pepper, according to their quantity. In brief, the vulgar notion of the term *poiſon* is erroneous: for by it is conceived that ſubſtances ſo called are in their nature poſitively deſtructive of life; but the truth is that the moſt virulent poiſons are, in all reaſon and fact, only deleterious according to the quantity applied. White arſenic ſwallowed in the quantity of ten grains or leſs, will deſtroy life; but in the quantity of one-ſixteenth of a grain, it is as harmleſs as a glaſs of wine; and further, in that doſe is a remedy for inveterate agues. Its quantity muſt be regulated as a manure: and it is hurtful only by exceſs, as food and condiments alſo are.

From theſe conſiderations I conclude, that there is no admiſſible contradictory evidence to the teſtimonies for the fer-

* "This is," ſays Mr. Willaume, "exactly the fact. This ſulphate of iron, the ſalt of peat, during the heat of the ſummer is frequently found in a chryſtalized ſtate, very white, and crackling under the feet; but is deliqueſcent in that form, and turns to its former dark colour when the air becomes moiſt."—*Note by Mr. J. W. Willaume.*

tilizing effect of ſulphate of iron, unleſs by ſuch contravening evidence the quantity ſtated to be uſed exceed fifty buſhels per acre; it being an eſtabliſhed fact, that in certain proportions this metallic ſalt is a poiſon to plants.

This diſcovery of Mr. Willaume will, I think, give new light, ſo as to explain fully the *rationale* of the improvement of land by the burnt earth and aſhes from paring and burning. It is uſual to account for the effects of this proceſs, by referring to ſuppoſed alkaline or other ſalts; but of theſe there is no evidence, nay, on trial I have not detected them, or at leaſt not in any efficient quantity; but this I know, that ſuch earth and aſhes contain *oxide of iron*, and as I ſuſpect of *manganeſe*; which from the analyſis, and the effect of ſalt of peat, muſt now be admitted into the claſs of manures. This very communication of Mr. Willaume, affords evidence of the truth of this conjecture, for the *aſhes of the peat which afford the ſalt* "have been long known as a manure, and the demand is on the increaſe:" of courſe, theſe aſhes contain an unuſual quantity of oxide of iron. A conſequence of this reaſoning is, that the burnt earth of ſoils will, *cæteris paribus*, fertilize in proportion to the oxide of iron it contains. Accordingly the aſhes of the peat, ſays Mr. Willaume, have a ſurpriſing effect, they "will double or treble a crop of any new-ſown graſs, ſuch as treſoil, &c." they are ſo beneficial, that in ſpite of the expence they are carried in bags by higlers to great diſtances. It would be extending this paper beyond the propoſed limits, to reaſon at a greater length, and to make a further induction of facts: therefore I will cloſe with aſſerting, that the more I contemplate the facts in Mr. Willaume's letter, the more evidence I perceive for the truth, that metallic ſalts, and metallic oxides in general, and ſalts and oxides of iron in particular, are manures, if applied in proper doſes.

It is conſidered as effective upon the ſame principle as condiments or ſeaſoners.

I do not think it is within the deſign of this paper to make obſervations on the anſwers to the 2d, 3d, 4th, 5th, and 6th queries, except, once for all deſiring that it may be underſtood, that I conſider the *ſalt of peat and the aſhes of peat*, as operating in promoting vegetation analogous to ſeaſoning, or condiments, taken with the food of animals; that is, analogous to muſtard, cinnamon, ginger, &c. which are not of themſelves at all or neceſſarily nutritious, but contribute to render other things nutritious, by exciting the action of the ſtomach

and

and other organs of digeſtion and aſſimulation. I have no doubt of the truth of the propoſition, that no living thing, neither plant nor animal, can grow and live in a ſtate of viſible action without conſtant ſupplies of *matter which has been alive;* in other words, *living* animals and vegetables can only live on *dead* animals and *dead* vegetables. No plant, nor animal has ever been known by experience, nor in the nature of things does it ſeem reaſonable, that they can be nouriſhed by mere water and pure air, as ſome perſons have aſſerted.

I ſhall make a few remarks on the other *two ſubſtances* which are the ſubject of Mr. Willaume's letter.

2. *The Peat.*

Chemical remark on peat.

The peat is a denſe maſs of vegetable matter for a certain depth, partly in a *dead* and partly in a *living* ſtate, with which is mixed more or leſs earth, and in burning it affords ſo much empyreumatic oil, as to give a diſagreeable taſte to roaſted proviſions; hence, as we are told, it has been rejected from the kitchen. This fuel affords a vaſt quantity of what the chemiſts call *lignic acid;* hence it is rejected alſo from the parlour, as very deſtructive to the grates. I beg to ſuggeſt that this lignic acid might be ſaved in burning the peat as fuel, and be uſed for various purpoſes in manufactures; and the charred peat may be uſed in place of charcoal of wood. Probably too other uſeful products will be found, on examining the matters more accurately which are afforded by diſtillation.

3. *Aſhes.*

The aſhe peat

If the peat were mere vegetable matter, the aſhes afforded by it would be as trifling as thoſe of wood; but ſome parts of the moor contain ſo much earth and oxide of iron, as to leave behind, on burning, a conſiderable quantity of incombuſtible matter; and ſuch kind of peat, we are told, is not uſed as fuel; but, after burning, the reſiduary matter is an efficacious manure, much more ſo than is commonly afforded by paring and burning. The aſhes are more red and more fertilizing than aſhes of common turf, becauſe they contain more iron.

produce clover.

The ſpontaneous ſpringing up of white clover, in land manured with theſe aſhes, is ſimilar to the ſpontaneous growth of this plant on heath land, which has been covered with lime to deſtroy all its preſent vegetation; and this fact ſhews that probably

Explanation.

bably these are seeds buried in the earth for many ages, which yet remain alive, but do not grow until exposed to the stimuli of air, water calorific, and lifeless animal, or vegetable matter.

(To be concluded in our next.)

XII.

Letter from Mr. ACCUM, *respecting an Error stated to exist in his Practical Chemistry.*

To Mr. NICHOLSON.

SIR,

Compton-Street, Soho, Feb. 16, 1805.

Omission stated to be made in Accum's chemistry.

W. F. C. censures me in your Journal, No. 38, page 105, of not having given in my book on Practical Chemistry "*The means of preparing either nitric acid, or nitrate of potash, or rather having stated them by implication, as incapable of being produced by art.*" The fidelity with which you have laid his remarks before the public, encourages me to hope, that with equal impartiality you will allow me to appear before the same tribunal, in order to plead to this accusation NOT GUILTY. For the method of obtaining nitric acid from its constituent principles, W. F. C. may read in the Vol. I. page 211.

In order to let the judicious readers who are not in possession of the work, judge for themselves, I beg leave to lay before them the method there pointed out, which literally runs thus:

Passage where the supposed omission is found.

"*Take a barometer tube, the diameter of which is about* $\frac{1}{6}$ *part of an inch. Shut one of its extremities with a cork, through the middle of which passes a small wire with a ball of metal at each end. Fill the tube with mercury and invert it into a bason of this fluid. Throw up into this tube as much of a mixture of* 12 *parts of nitrogen gas, and* 87 *parts of oxigen gas as will fill* 3 *inches. Through this gas by means of the wire in the cork pass a number of electric sparks; the volume of the gas gradually diminishes, and in its place will be found nitrous acid.*" *

* *Nitrous acid* does not differ from *nitric acid*, in composition, but merely by the admixture of nitrous gas. See the book under consideration, Vol. II. p. 288.

It

It appears that W. F. C. has not noticed theſe lines. The production of nitric acid by ſyntheſis, being here clearly ſtated, his animadverſion relative to that ſubject falls to the ground.

To exculpate myſelf from his further accuſation, namely, of not having noticed the production of nitrate of potaſh by artificial means, or rather having ſtated it by implication, will become equally erroneous, on reminding him, that the method of preparing this ſalt artificially is pointed out, Vol. II. page 287, thus, "*Nitrate of potaſh may be prepared by neutralizing carbonate of potaſh with nitric acid.*" Other explanations.

From what has been ſtated, it is obvious that W. F. C.'s remarks are not correct, for both the methods of obtaining nitric acid, and nitrate of potaſh are correctly given, and not by implication, as W. F. C. apprehends.

I have the honour to be, Sir,

Your's,

FREDERICK ACCUM.

XIII.

Proceſſes for obtaining a durable and ſuperior Lake from Madder.

By Sir H. C. Englefield, *M. P. F. R. S.**

THE want of a durable red colour, which ſhould poſſeſs ſomething of the depth and tranſparency of the lakes made from cochineal, firſt induced me to try whether the madder root, which is well known to furniſh a dye leſs ſubject to change by expoſure to air, than any other vegetable colour, except indigo, might not produce ſomething of the colour I wanted. Introduction.

Several of the moſt eminent painters of this country have, for ſome time, been in the habit of uſing madder lakes in oil pictures: but the colours they poſſeſſed under this name were either a yellowiſh red, nearly of the hue of brick-duſt, or a pale pink opake, and without clearneſs or depth of tint, and quite unfit to be uſed in water-coloured drawing, which was the principal object of my ſearch. Madder lakes have been already uſed; but they were bad.

My firſt attempts were to repeat the proceſs given by Margraf, in the Memoirs of the Academy of Berlin: but the co- Margraf's proceſs affords a weak colour,

* From the Tranſactions of the Society of Arts, who voted the gold medal in honour of this diſcovery. 1804.

lour

lour produced by this mode was of a pale red, and very opake, although the eminent author of the proceſs ſtates the colour he produced to be that of "*le ſang enflammé*," which probably means a deep blood colour. It may, however, be obſerved, that colours prepared with a baſis of alumine will appear much deeper when ground in oil than they do in the lump, the oil rendering the alumine nearly tranſparent. This advantage is however, loſt in water-colours. On examining the reſiduum of the madder root, after it had been treated in Margraf's method, it appeared tinged with ſo rich a red, that it was obvious, that by far the greater part of the colour ſtill remained in it, and that the moſt powerful and beautiful part. To extract this, ſeveral ineffectual trials were made, which it would be uſeleſs to enter into; but, on attentively examining the appearances which took place on infuſing the madder in water, I began to ſuſpect that the red colouring matter was very little, if at all, ſoluble in water, and that it was only mechanically mixed with the water when poured on the root, and ſuſpended in it by the mucilage, with which the root abounds.

becauſe moſt of the colour remained in the root.

Water takes up little of the colour.

A very ſmall quantity, therefore, can be obtained by any infuſion or decoction, as the greater part ſinks down on the root, or remains with it on the ſieve, or in the bag, through which the infuſion or decoction is paſſed to render it clear. I therefore was induced to try whether, by ſome merely mechanical means, I could not ſeparate the colouring matter from the fibrous part of the root. In this attempt my ſucceſs was fully equal to my hopes; and, after ſeveral trials, I conſider the proceſs I am now about to deſcribe, as the moſt perfect I have been able to diſcover.

Whence mechanical preſſure is adviſable.

Proceſs 1. Two ounces of madder were pounded in a bag with a pint of water;

Proceſs 1. Encloſe two ounces, troy weight, of the fineſt Dutch madder, known in commerce by the name of crop madder, in a bag, capable of containing three or four times that quantity, and made of ſtrong and fine calico. Put it into a large marble, or porcelain mortar, and pour on it about a pint of cold ſoft water. The Thames water, when filtered, is as good as can be uſed; it being very nearly as pure as diſtilled water, at leaſt when taken up a very little way above London. With a marble or porcelain peſtle, preſs the bag ſtrongly in every direction, and, as it were, rub and pound it as much as can be done without endangering the bag. The water will very ſoon be loaded with the colouring matter, ſo

as

as to be quite opake and muddy. Pour off the water, and add another pint of freſh water to the root, agitating and triturating it in the manner before deſcribed; and repeat the operation till the water comes off the root very ſlightly tinged. About five pints of water, if well agitated and rubbed, will extract from the root nearly the whole of its colour; and if the reſidual root be taken out of the bag and dried, it will be found to weigh not more than five drachms, apothecaries weight; its colour will be a kind of light nankeen, or cinnamon, and it will have entirely loſt the peculiar odour of the root, and only retain a faint woody ſmell.

The water when turbid with colour is poured off and another pint added; and rubbed as before:

Five ſucceſſive pints extract all the colour, leaving five drams of fibre.

The water loaded with the colouring matter, muſt be put into an earthen or well-tinned copper, or, what is ſtill better, a ſilver veſſel, (for the uſe of iron muſt be carefully avoided through the whole), and heated till it juſt boils. It muſt then be poured into a large earthen or porcelain baſon, and an ounce troy weight of alum diſſolved in about a pint of boiling ſoft water, muſt be poured into it, and ſtirred until it is thoroughly mixed. About an ounce and a half of a ſaturated ſolution of mild vegetable alkali ſhould be gently poured in, ſtirring the whole well all the time. A conſiderable effervescence will take place, and an immediate precipitation of the colour. The whole ſhould be ſuffered to ſtand till cold; and the clear yellow liquor may then be poured off from the red precipitate. A quart of boiling ſoft water ſhould again be poured on it, and well ſtirred. When cool, the colour may be ſeparated from the liquor by filtration through paper in the uſual way; and boiling water ſhould be poured on it in the filter, till it paſſes through of a light ſtraw colour, and quite free from any alkaline taſte. The colour may now be gently dried; and when quite dry, it will be found to weigh half an ounce; juſt a fourth part of the weight of the madder employed.

The coloured water is made boiling hot;

and one oz. of alum diſſolved in a pint of boiling water added; $1\frac{1}{2}$ oz. of mild veg. alk. afford a precipitate of colour.

Edulcoration, filtering and drying afford half an ounce of lake,

By analyſis, this colour poſſeſſes rather more than 40 per cent. of alumine. If leſs than an ounce of alum be employed with two ounces of madder, the colour will be rather deeper; but if leſs than three quarters of an ounce be uſed, the whole of the colouring matter will not be combined with alumine. On the whole, I conſider the proportion of an ounce of alum to two ounces of madder, as the beſt.

which contains two fifths of alumine.

Proceſs 2. If, when the ſolution of alum is added to the water loaded with the colouring matter of the root, the whole be

Proceſs 2. If the liquor of proceſs 1. be ſuffered to cool

and ſettle after the alum was added it affords a dull red ſediment. and the remaining fluid if heated and precipitated by alkali, affords a fine but not ſolid lake. This is Watt's proceſs.

be ſuffered to ſtand, without the addition of the alkali, a conſiderable precipitation will take place, which will be of a dark dull red. The remaining liquor, if again heated, will, by the admiſſion of the alkali, produce a roſe-coloured precipitate of a beautiful tint, but wanting in force and depth of tone.

This is the proceſs recommended by Mr. Watt, in his Eſſay on Madder, in the *Annals de chymie, Tome* 7; and this latter colour is what may perhaps, with propriety, be called Madder Lake. But, although the lighter red may be excellent for many purpoſes, yet I conſider the colour produced by the union of the two colouring matters, as given in the firſt proceſs, as far preferable for general uſe, being of a very beautiful hue when uſed thin, and poſſeſſing unrivalled depth and richneſs either in oil or water, when laid on in greater body.

Leſs alum cauſes the ſecond precipitate to be leſs in quantity, but richer.

If but half an ounce of alum be added to the two ounces of the root, the firſt precipitate will be nearly ſimilar to that when an ounce is employed; but the ſecond, or lake precipitate, will be leſs in quantity, and of a deeper and richer tint. In this caſe the whole of the colouring matter, as before obſerved, is certainly not combined with the alumine; for, on adding more alum to the remaining liquor, a precipitate is obtained of a light purpliſh red. In this proceſs, when two ounces of madder and an ounce of alum are uſed, the firſt precipitate has about 20 per cent. of alumine, and the ſecond, or lake precipitate, about 53 per cent; but theſe proportions will vary a little in repetitions of the proceſs.

Proceſs 3. When the waſhing is performed by boiling inſtead of cold water, the colour is neither ſo good nor ſo plentiful.

Proceſs 3. If the madder, inſtead of being waſhed and triturated with cold water, as directed in the foregoing proceſs, be treated in exactly the ſame manner with boiling water; the colour obtained will be rather darker, but ſcarcely of ſo good a tint; and the reſiduum of the root, however carefully preſſed and waſhed, will retain a ſtrong purpliſh hue; a full proof that ſome valuable colour is retained in it, probably fixed in the woody fibre by the action of heat. Mr. Watt, in his excellent treatiſe on madder above mentioned, obſerves, that cold water extracts the colour better than hot water; and I have reaſon to ſuſpect, that a portion of that colouring matter, which produces the bright red pigment, diſtinguiſhed before by the name of Madder Lake, remains attached to the root, when acted on by boiling water.

Proceſs 4.

Proceſs 4. If to two ounces of madder, a pint of cold water be added, and the whole be ſuffered to ſtand for a few days (three or four days) in a wide-mouthed bottle, lightly corked, in a temperature of between 50° and 60°, and often ſhaken; a ſlight fermentation will take place, the infuſion will acquire a vinous ſmell, and the mucilaginous part of the root will be in a great degree deſtroyed, and its yellow colour much leſſened. If the whole be then poured into a calico bag, and the liquor be ſuffered to drain away without preſſure, and then the root remaining in the bag be treated with cold water, &c. exactly as directed in the firſt proceſs, the red colouring matter will quit the root with much greater eaſe than before fermentation. It will alſo be equal in quantity to that afforded by the firſt proceſs, but of a much lighter red. This difference of tint appears to be owing to a deſtruction of a part of the lake by the fermentation of the root; for if the colours from the fermented root be obtained ſeparate, as in Proceſs 2, the firſt precipitate will not ſenſibly differ from that obtained from the unfermented madder, but the ſecond, or lake will be of a very light pink. This proceſs, then, is not to be recommended.

Proceſs 4. If the madder be ſuffered to ſtand before waſhing for ſome days in cold water, the mucilage is deſtroyed by fermentation, and the colour more eaſily extracted; but it is not ſo good.

Spaniſh and Smyrna Madders.

Spaniſh Madder affords a colour of rather a deeper tone than the Dutch Madder, but it does not appear to be of ſo pure a red as the Zealand Crop Madder.

Spaniſh madder affords a deeper but leſs pure colour than Dutch madder.

The Smyrna Madder is a very valuable root. The colour produced from it by Proceſs 1, is of a deeper and richer tint than any I have obtained from the Dutch Madder. The quantity produced from two ounces, is only three drachms, twenty-four grains: but this is not to be wondered at; for as this madder is imported in the entire root in a dry ſtate, and the Crop Madder of Zealand conſiſts principally of the bark, in which probably the greateſt part of the colouring ſubſtance reſides, there is every reaſon to think that the Smyrna madder really contains a greater proportion of colour than the Zealand, in equal weights of the entire root.

Smyrna madder is better than Dutch.

The products of Proceſs 2 prove, that the *lake* of the Smyrna madder is more abundant in quantity, and of a richer tone than that of the Dutch root; for, from two ounces of Dutch madder the firſt precipitate was two drachms, and the lake was two drachms and forty eight grains; whereas, from

two ounces of the Smyrna root the firſt precipitate was one drachm and twenty four grains, and the lake was two drachms and twenty-four grains. The proportion of the lake to the other colour is, therefore, much higher in the Smyrna, than in the Dutch root.

Freſh Madder.

The recent root is preferable.

The colour may be prepared from the recent root; and it will be of a quality equal, if not ſuperior, to any other. The difficulty of procuring the freſh root has prevented me from making as many experiments on it as I could have wiſhed. I procured, however, a ſmall quantity of the beſt roots packed in moſs from Holland, and the following proceſs anſwered perfectly well.

Experiment according to proceſs 1. with freſh madder;

Eight ounces of the root having been firſt well waſhed and cleaned from dirt of all kinds, were broken into ſmall pieces, and pounded in a bell-metal mortar, with a wooden peſtle, till reduced into an uniform paſte. This paſte being encloſed in a calico bag, was waſhed and triturated, as deſcribed in the firſt proceſs, with cold water. About five pints ſeem to have extracted nearly the whole of the colour. To the water thus loaded with colour, and boiled as before, one ounce of alum, diſſolved in a pint of boiling water, was added, and the alkali poured on the whole, till the taſte of the mixture was juſt perceptibly alkaline. The colour thus obtained, when dry, was of a very beautiful quality.

very ſucceſsful.

It promiſes great advantage to the arts.

The ſucceſs of this experiment, which was twice repeated with the ſame reſult, has led me to hope, that it is not impoſſible that the mode of obtaining the colour from the freſh root here deſcribed, may be productive of advantages for more extenſive uſe than I had in view when firſt I attempted to obtain a pigment from madder. Many tracts of land in this country are as well adapted to the growth of this valuable article, as the ſoil of Holland can be; and the cultivation of it, which has more than once been attempted to a conſiderable extent, has been laid aſide, principally from the expence attendant on the erection of drying-houſes and mills, and the great expence and nicety requiſite for conducting the proceſs of drying. But ſhould the colour prepared in the mode juſt deſcribed, be found to anſwer the purpoſes of the dyers and calico-printers, the proceſs is ſo eaſy, and the apparatus re-

For if the colour ſhould anſwer for dying, &c. it would ſave drying and carriage;

quired

quired for it ſo little expenſive, that it might be in the power of any grower of the root to extract the colour: beſides which, another great advantage would be obtained; the colour thus ſeparated from the root, may be kept any length of time, without danger of ſpoiling, and its carriage would be only one fourth of that of the root. I am, moreover, thoroughly inclined to believe, that in the preſent mode of uſing the root, a very conſiderable part of the colour is left in it by the dyers; and, ſhould this prove to be the caſe, an advantage much greater than any hitherto adverted to, may ariſe from the proceſs here recommended.

may be kept for a long time,

and probably be more effective.

Should it be attempted to obtain the colour from the freſh root, on an extenſive ſcale, I ſhould recommend, that the root be firſt reduced to as uniform a pulp as poſſible, by grinding or pounding. To this purpoſe, it is probable that the cyder-mill would anſwer perfectly well; and its extreme ſimplicity is a great recommendation. For the purpoſe of trituration, bags of woolen, ſuch as are uſed in the oil-mills, would probably anſwer as well as calico, and they would be much cheaper and more durable. A large vat, with ſtampers, would be eaſily conſtructed, by thoſe who are converſant in mechanics, for the holding them and preſſing them in water; and when the colour was boiled and precipitated, the flues of the boilers might eaſily be formed into convenient drying tables, without any additional expenſe of fuel. The part of the proceſs, which I conſider as of the greateſt importance, and as being the eſſential advantage of my methods over all thoſe which have come to my knowledge, is the trituration or preſſing of the root in water; and I believe that the colouring matter of the root has not been hitherto conſidered as ſo nearly inſoluble in water, as I have reaſon to think it is.

Directions for eſtabliſhing a manufactory of madder colour.

It were much to be wiſhed, that in the preſent advanced ſtate of Chemiſtry, ſome ſkilful analyſer would inveſtigate the properties of this very uſeful root; in which perhaps it will be found, that there are three, if not four, different colouring ſubſtances. Such are the proceſſes and views, which I have thought it not improper to ſubmit to the conſideration of the Society of Arts, &c.

We want a good analyſis of madder.

I have only now to deſcribe the ſpecimens which accompany this paper; aſſuring the Society, that they have been all prepared by my own hands entirely, and that I am therefore

The ſpecimens exhibited to the Society.

reſponſible

reſponſible for their having been produced by the proceſſes ſtated, without the addition of any foreign matter whatever, excepting the cake ground up with gum, and the bladder of oil-colour, which were prepared from the colour which I gave him, by Mr. Newman, of Soho-Square, whoſe ſkill and fidelity are too well known to need any teſtimony in their favour.

It may be proper to add, that all the colours produced from the Dutch madder were prepared from the ſame parcel of crop madder, in order that the differences in them might proceed from the proceſſes, and not from a variation in the qualities of the root, which, in different ſpecimens, will produce different ſhades of colour under the ſame mode of treatment.*

1. Dutch madder, treated by proceſs 1ſt.
2. Ditto - - - - proceſs 2d.
3. Ditto - - - - proceſs 3d.
4. Ditto - - - - proceſs 4th.
5. Dutch madder, two ounces; alum, half an ounce; treated by proceſs 2.
6. Dutch madder, two ounces; alum, one ounce; fermented two days, and then treated by proceſs 2.
7. Produce of proceſs 1, ground in gum by Mr. Newman.
8. Produce of proceſs 1, ground in oil by Mr. Newman.

S—1. Smyrna madder, by proceſs 1.
S—2. Ditto - - - proceſs 2.
S—3. Ditto - - - proceſs 3.
S—4. Ditto - - - proceſs 4.

* Certificates accompanied the foregoing deſcription, from Mr. Cotman and Mr. Munn, teſtifying the merits of Sir H. Englefield's madder lakes, as water colours; and alſo, from Meſſrs. Weſt, Trumbull, Opie, Turner, Daniel, and Hoppner, ſpeaking greatly in its favour, where it has been tried in oil-colours.

The

XIV.

The Dutch Method of curing Herrings, extracted and tranſlated from the German of Krünitz's Economical Encyclopædia (Oeconomiſche Encyclopädie), *Article Häring, by* J. HINCKLEY, *Eſq. F. S. A.* *

Dutch method of catching and curing of herrings.

THE veſſels employed in this fiſhery, commonly called herring-buſſes, from the Dutch name, are generally between 48 and 60 tons burthen, though ſome from 40 to 80 and 100 tons are uſed. The largeſt of all carry 120 tons, are three-maſted veſſels, with one deck, and a cabin at each end; that a-head ſerving as a kitchen. Of the larger, the crews are 24 men, thoſe of the ſmaller 18. They carry a few ſmall guns and muſquetry.

Their nets are dipped, or caſt out, in the evening, and drawn up in the morning. It requires three hours to wind them on board. From the net, the fiſh are immediately put into baſkets, while others of the crew are occupied till evening in gutting, ſalting, and packing. But although from ten to fifteen laſt are ſometimes taken at a draught, the twelve perſons uſually employed for the purpoſe, cannot complete more than five laſt in a day.

During the three firſt weeks, from the 25th of June to the 16th of July, all the freſh-caught herrings are thrown into caſks without picking, and conveyed to Holland, in the jägers, or yachts †, that accompany the herring-buſſes. But, after this period, immediately on being got on board and gutted, they are aſſorted into three qualities,—maiden her-

* From the Tranſactions of the Society of Arts, who have publiſhed it for the information of our fiſhers in North Britain, who appear to be well acquainted with the treatment from catching and landing, but not with the ſubſequent proceſſes. Mr. Walter Baine, of Greenock, had the Society's ſilver medal for this object laſt ſeaſon.

† Theſe are ſmall faſt-ſailing veſſels, which follow the herring-buſſes, ſupply them with proviſions, caſks, ſalt, and other neceſſaries, and carry the fiſh that have been taken to the neareſt port, where they are re-packed, and prepared for ſending to the places of their deſtination.

rings,

Dutch method of catching and curing of herrings.

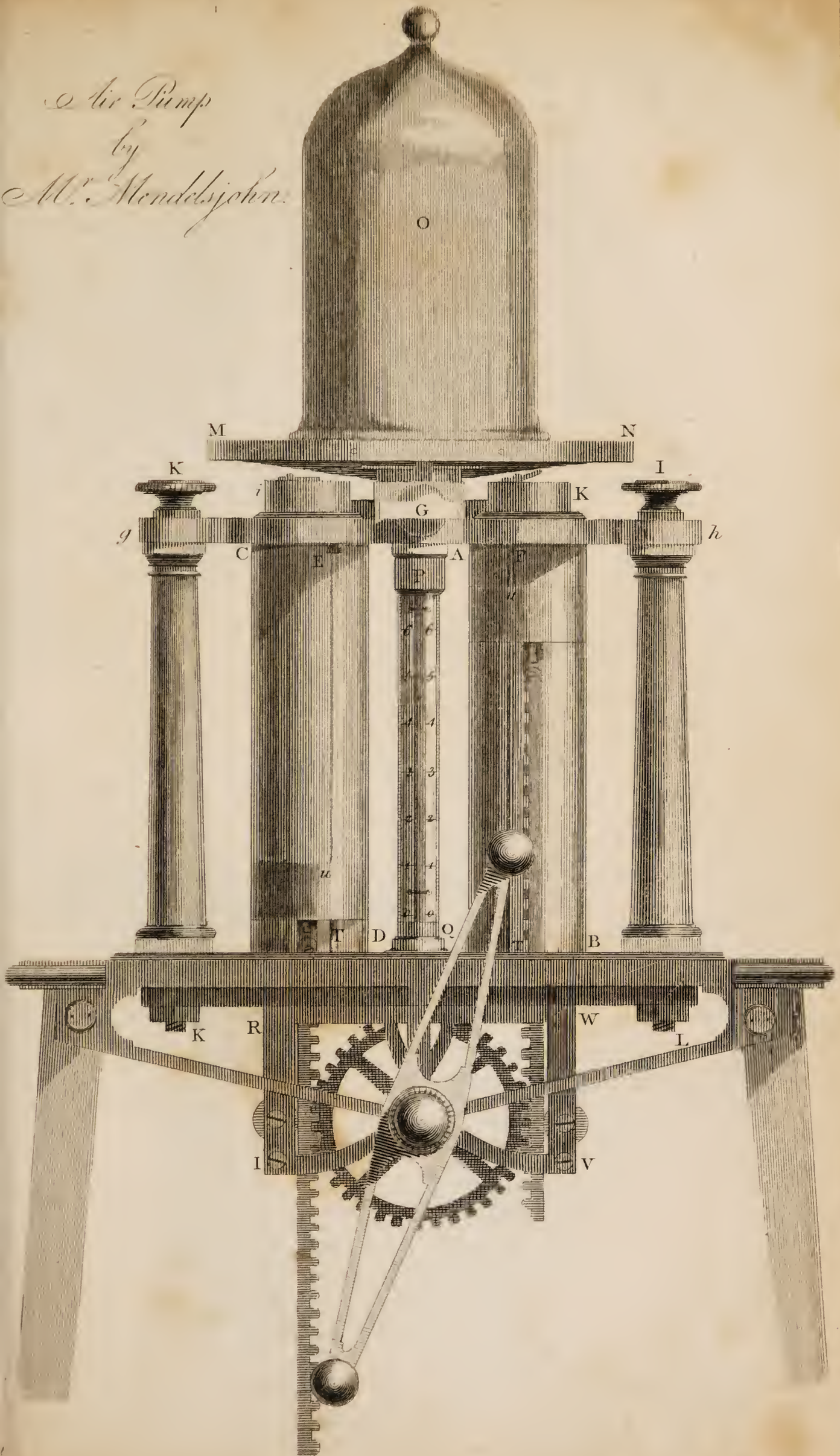
rings, full herrings, and ſhot herrings. The firſt of theſe are thoſe taken earlieſt, and without row or melt, but which, though well flavoured, do not keep. Full herrings are thoſe taken at Midſummer, on the point of ſpawning; from which the brand-herrings, ſo called from the barrels being marked with a hot iron, only differ in being caught later, re-packed immediately on arrival, and ſo cloſe and hard preſſed down, that they do not require re-packing at other places, but only new pickle; and are immediately expedited, or may remain on hand: whereas the other two ſorts, not being ſo cloſely laid, muſt abſolutely be re-packed. Shot-herrings are thoſe which have ſpawned, or are taken in the act of ſpawning, in conſequence of which they are thin and lean.

With the laſt two ſorts the buſſes themſelves return (as ſoon as they have got their loading, or find no more fiſh), one after another, to port, where all three ſorts, except the brand-herrings, before being expedited, are opened, ſalted anew, re-packed, and ſo heaped up, that fourteen caſks are re-packed in twelve, which make a laſt. By a regulation of the States-General, this re-packing muſt be performed in the open air, where ſtrict watch is kept, that the ſpoiling fiſh be carefully ſeparated from the good, and the latter properly laid in the barrels, and ſtrongly preſſed down.

The Dutch fiſhery continues generally from twenty to twenty-ſix weeks, or even ſomewhat longer, namely, from the 25th of June to the middle of January. The Dutch fiſh only on the Scotch and Engliſh coaſts, off Hittland, Fairhill, and Bocken, from Midſummer till the 25th of July; off Bocken or Serenial, from thence till the 14th of September; and in deep water, Eaſt of Yarmouth, and as far as the mouth of the Thames, from thence to the 25th of November, when the regular fiſhery ceaſes. But herrings are found not far from Yarmouth till the end of January, after which the fiſhery is prohibited, as the ſpawning ſeaſon then commences.

(To be continued.)

Air Pump by Mr. Mendelsjohn.



Mr Gough's Theory of the Speaking Trumpet.

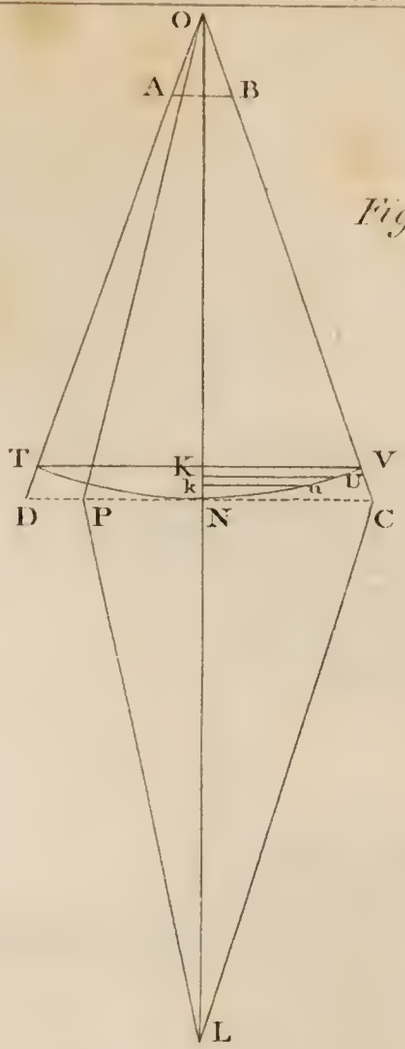


Fig. 3.

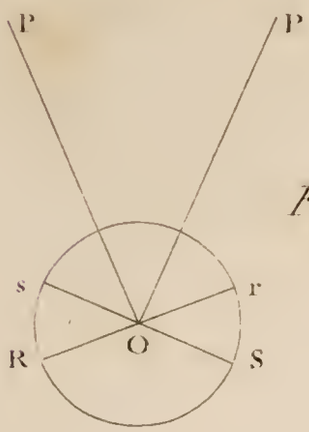


Fig. 4.

Part of the Air Pump.

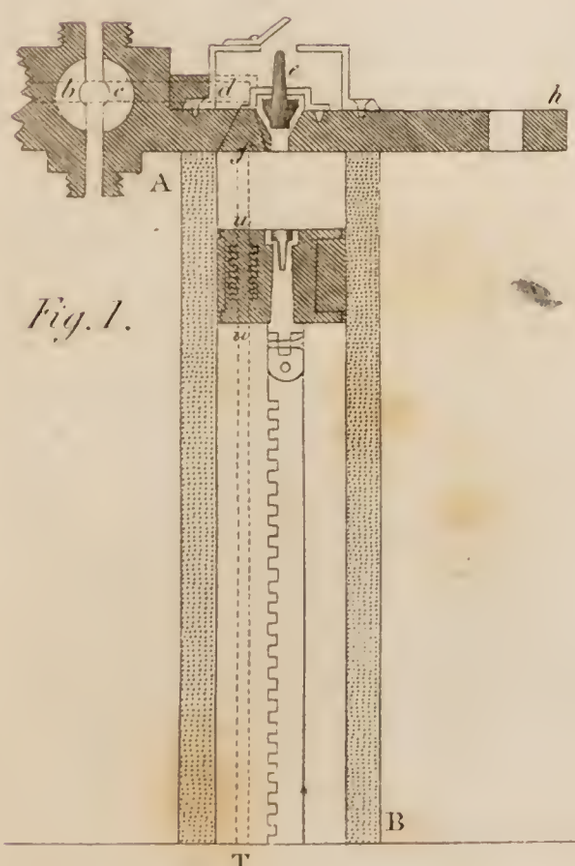


Fig. 1.

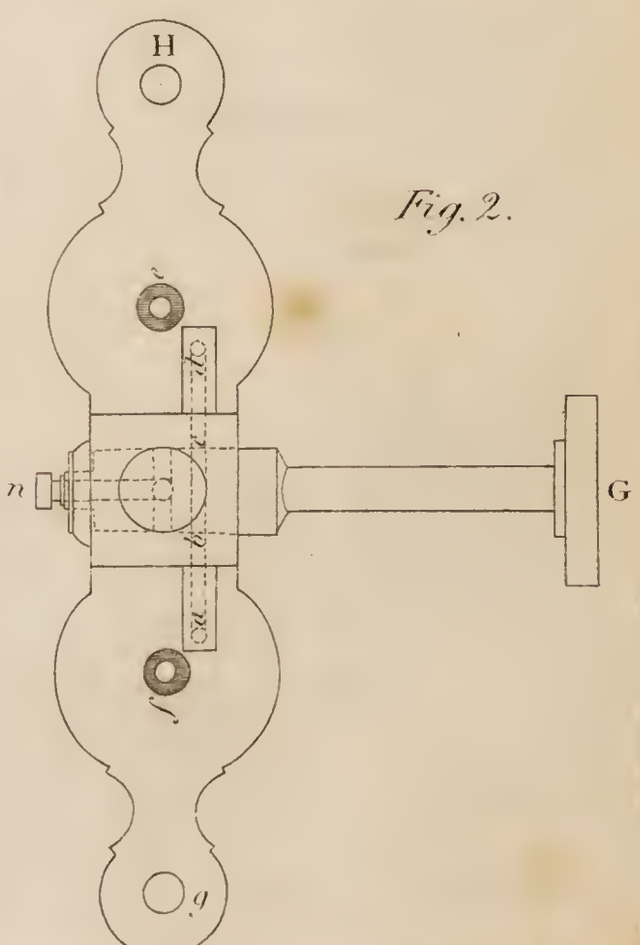


Fig. 2.

Fig. 1.

Improvement in Woulfe's Apparatus.

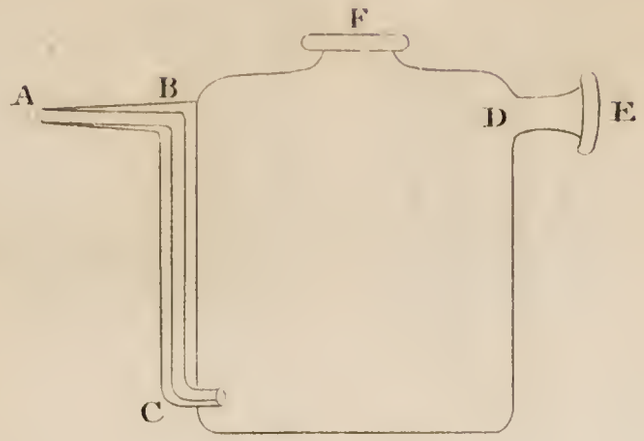


Fig. 3.

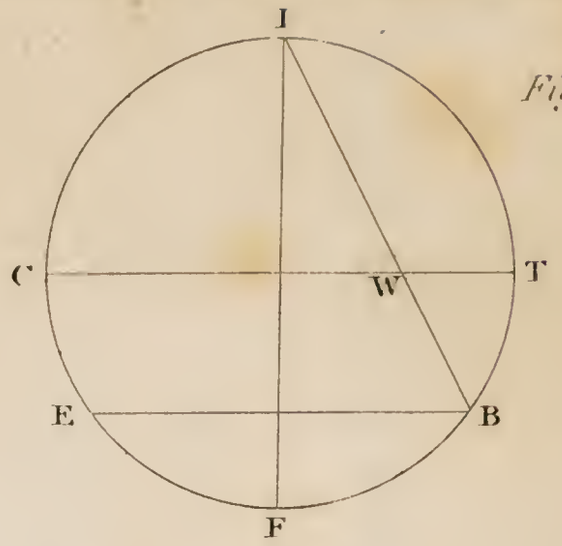
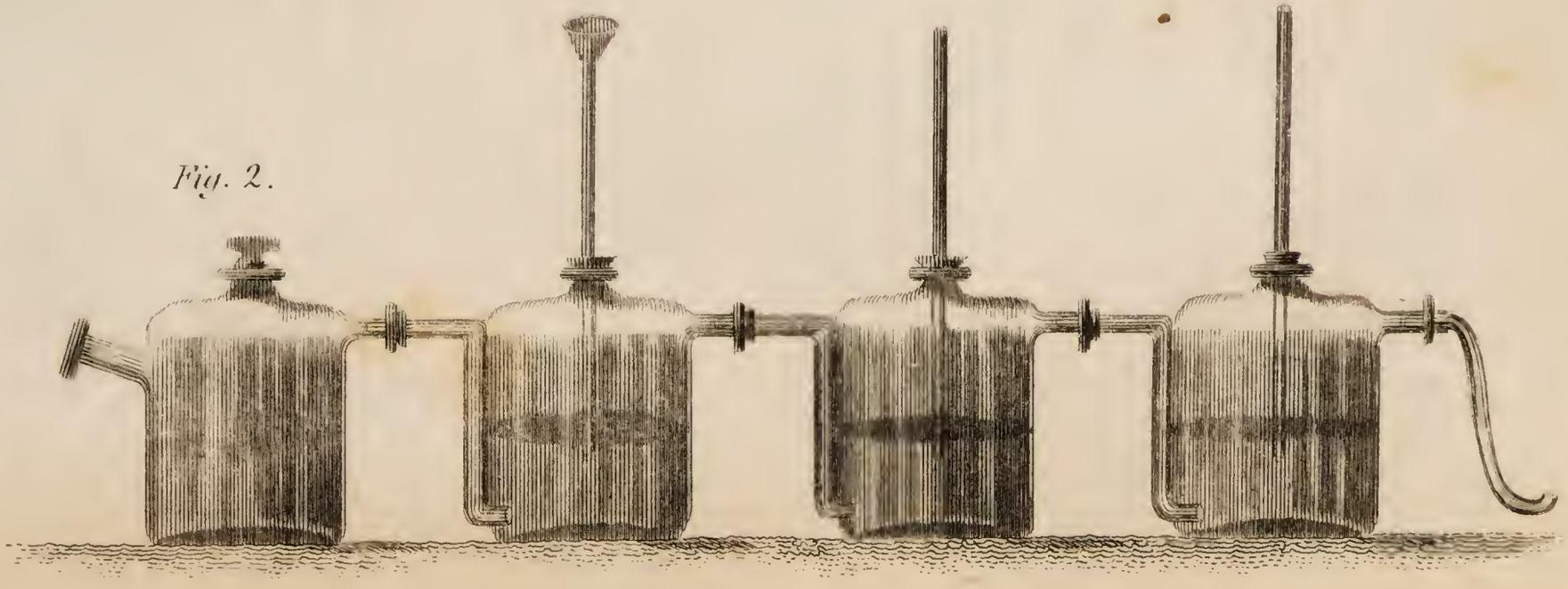


Fig. 2.



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Mudow Sc. Rupsell Sc.

A

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OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

APRIL, 1805.

ARTICLE I.

A ſhort Account of the Cauſe of the Diſeaſe in Corn, called by Farmers, the Blight, the Mildew and the Ruſt. Reprinted, and the Plates copied, with Permiſſion, from a Memoir communicated by the Right Hon. SIR JOSEPH BANKS, *Bart. P. R. S. &c. &c. &c. With ſome additional Notes.**

BOTANISTS have long known that the Blight in Corn is occaſioned by the growth of a minute paraſitic fungus or muſhroom on the leaves, ſtems, and glumes of the living plant. Felice

Botaniſts have long known that the blight is cauſed by a minute paraſitic fungus;

* Immediately after the title in the original is the following prefatory note:

"The following brief Publication, ſuggeſted by the alarming "ſtate of the harveſt in Auguſt laſt, would have been diſtributed "before the end of wheat ſeed-time, had the engraver fulfilled his "engagement.

"This circumſtance will, it is hoped, be conſidered as a ſufficient "apology for the want of actual obſervations on the origin and pro-"greſs of the diſeaſe. Theſe it is preſumed will be abundantly "ſupplied in the courſe of the preſent year, by thoſe intelligent "agriculturiſts, whoſe reſidence in the country enables them daily "to examine, not only the progreſs of their crops, but the origin "and advances alſo of all thoſe obſtacles which nature has oppoſed to the ſucceſs of agricultural labours, as if to awaken the ener-

Felice Fontana publiſhed in the year 1767 an elaborate account of this miſchievous weed,* with microſcopic figures, which give a tolerable idea of its form; more modern botaniſts† have given figures both of corn and of graſs affected by it, but have not uſed high magnifying powers in their reſearches.

but agriculturiſts have paid little attention to this fact.

Agriculturiſts do not appear to have paid, on this head, ſufficient attention to the diſcoveries of their fellow-labourers in the field of nature; for though ſcarce any Engliſh writer of note on the ſubject of rural economy has failed to ſtate his opinion of the origin of this evil, no one of them has yet attributed it to the real cauſe, unleſs Mr. Kirby's excellent papers on ſome diſeaſes of corn, publiſhed in the Tranſactions of the Linnæan Society, are conſidered as agricultural eſſays.

Engravings of this deſtructive plant from accurate drawings by Mr. Bauer are here offered to their conſideration.

On this account it has been deemed expedient to offer to the conſideration of farmers, engravings of this deſtructive plant, made from the drawings of the accurate and ingenious Mr. Bauer, Botanical Painter to his Majeſty, accompanied with his explanation, from whence it is preſumed an attentive reader will be able to form a correct idea of the facts intended to be repreſented, and a juſt opinion whether or not they are, as is preſumed to be the caſe, correct and ſatisfactory.

Organized ſtructure of the ſurface of ſtraw. Its pores or mouths, ſhut in dry and open in wet weather.

In order, however, to render Mr. Bauer's explanation more eaſy to be underſtood, it is neceſſary to premiſe, that the ſtriped appearance of the ſurface of a ſtraw which may be ſeen with a common magnifying glaſs, is cauſed by alternate longitudinal partitions of the bark, the one imperforate, and the other furniſhed with one or two rows of pores or mouths, ſhut in dry, open in wet weather, and well calculated to imbibe fluid whenever the ſtraw is damp. ‡

By

" gies of reaſon, and to reward the farmer for the exertions of his " intellectual faculties, by the ſatisfaction of ſurmounting them."

" Jan. 30th, 1805." " JOS. BANKS."

* Oſſervazioni ſopra la Ruggine del Grano. Lucca, 1767, 8vo.

† Sowerby's Engliſh Fungi, Vol. II. Tab. 140, Wheat Tab. 139. Poa aquatica.

The pores or mouths on the ſurface of plants are deſigned to abſorb moiſture.

‡ Pores or mouths ſimilar to theſe are placed by nature on the ſurface of the leaves, branches, and ſtems, of all perfect plants, a proviſion intended no doubt to compenſate, in ſome meaſure, the want of loco-motion in vegetables. A plant cannot when thirſty

go

By theſe pores, which exiſt alſo on the leaves and glumes, it is preſumed that the ſeeds of the fungus gain admiſſion, and at the bottom of the hollows to which they lead, (ſee Plate IX. Fig. 1, 2,) they germinate and puſh their minute roots, no doubt (though theſe have not yet been traced) into the cellular texture beyond the bark, where they draw their nouriſhment, by intercepting the ſap that was intended by nature for the nutriment of the grain; the corn of courſe becomes ſhrivelled in proportion as the fungi are more or leſs numerous on the plant; and as the kernel only is abſtracted from the grain, while the cortical part remains undiminiſhed, the proportion of *flour to bran* in blighted corn, is aways reduced in the ſame degree as the corn is made light. Some corn of this year's crop will not yield a ſtone of flour from a ſack of wheat; and it is not impoſſible that in ſome caſes the corn has been ſo completely robbed of its flour by the fungus, that if the proprietor ſhould chooſe to incur the expenſe of thraſhing and grinding it, bran would be the produce, with ſcarce an atom of flour for each grain.

The ſeeds of the fungus enter the pores,

germinate in the cellular texture, intercept the ſap, and render the grain ſhriveiled by abſtraction of the kernel.

The flour may thus be almoſt totally intercepted.

Every ſpecies of corn, properly ſo called, is ſubject to the blight; but it is obſervable that ſpring corn is leſs damaged by it than winter, and rye leſs than wheat, probably becauſe it is ripe and cut down before the fungus has had time to increaſe in any large degree.—Tull ſays that "white cone or "bearded wheat, which hath its ſtraw like a ruſh full of pith, "is leſs ſubject to blight than Lammas wheat, which ripens a "week later." See page 74. The ſpring wheat of Lincolnſhire was not in the leaſt ſhrivelled this year, though the ſtraw was in ſome degree infected: the millers allowed that it was the beſt ſample brought to market. Barley was in ſome places conſiderably ſpotted, but as the whole of the ſtem of that grain is naturally enveloped in the hoſe or baſis of the leaf, the fungus can in no caſe gain admittance to the ſtraw; it is how-

All corn is liable to the blight; ſpring corn leſs than winter, and rye leſs than wheat.

Hiſtorical facts.

go to the brook and drink, but it can open innumerable orifices for the reception of every degree of moiſture, which either falls in the ſhape of rain and of dew, or is ſeparated from the maſs of water always held in ſolution by the atmoſphere; it ſeldom happens in the drieſt ſeaſon, that the night does not afford ſome refreſhment of this kind, to reſtore the moiſture that has been exhauſted by the heats of the preceding day.

ever to be obſerved that barley riſes from the flail lighter this year than was expected from the appearance of the crop when gathered in.

Little information was obtained by enquiry reſpecting blight; but its cauſe being now explained, the remedy it is hoped will be diſcovered.

Though diligent enquiry was made during the laſt autumn, no information of importance relative to the origin or the progreſs of the blight could be obtained: this is not to be wondered at; for as no one of the perſons applied to had any knowledge of the real cauſe of the malady, none of them could direct their curioſity in a proper channel. Now that its nature and cauſe have been explained, we may reaſonably expect that a few years will produce an intereſting collection of facts and obſervations, and we may hope that ſome progreſs will be made towards the very deſirable attainment of either a preventive or a cure.

Progreſs of the infection.

It ſeems probable that the leaf is firſt infected in the ſpring, or early in the ſummer, before the corn ſhoots up into ſtraw, and that the fungus is then of an orange colour;* after the ſtraw has become yellow, the fungus aſſumes a deep chocolate brown: each individual is ſo ſmall that every pore on a ſtraw will produce from 20 to 40 fungi, as may be ſeen in the plates, and every one of theſe will no doubt produce at leaſt 100 ſeeds; if then one of theſe ſeeds tillows out into the number of plants that appear at the bottom of a pore in *Plate* IX. *Fig.* 1, 2. how incalculably large muſt the increaſe be! A few diſeaſed plants ſcattered over a field muſt very ſpeedily infect a whole neighbourhood, for the ſeeds of fungi are not much heavier than air, as every one who has trod upon a ripe puff-ball muſt have obſerved by ſeeing the duſt, among which is its ſeed, riſe up and float on before him.

Increaſe of the fungus incalculably rapid;

and its periods of generation very quick.

How long it is before this fungus arrives at puberty, and ſcatters its ſeeds in the wind, can only be gueſſed at by the analogy of others; probably the period of a generation is ſhort, poſſibly not more than a week in a hot ſeaſon: if ſo, how frequently in the latter end of the ſummer muſt the air be loaded as it were with this animated duſt, ready, whenever a

* The Abbé Teſſier, in his *Traité des Maladies des Grains*, tells us that, in France, this diſeaſe firſt ſhews itſelf in minute ſpots of a dirty white colour on the leaves and ſtems, which ſpots extend themſelves by degrees, and in time change to yellow, and throw off a dry orange coloured powder. pp. 201, 340. *Note of Sir J. B*

gentle

gentle breeze, accompanied with humidity, ſhall give the ſignal to intrude itſelf into the pores of thouſands of acres of corn. Providence, however, careful of the creatures it has created, has benevolently provided againſt the too extenſive multiplication of any ſpecies of being; was it otherwiſe, the minute plants and animals, enemies againſt which man has the feweſt means of defence, would increaſe to an inordinate extent; this however, can in no caſe happen, unleſs many prediſpoſing cauſes afford their combined aſſiſtance. But for this wiſe and beneficent proviſion, the plague of ſlugs, the plague of mice, the plagues of grubs, wire-worms, chafers, and many other creatures whoſe power of multiplying is countleſs as the ſands of the ſea, would, long before this time, have driven mankind, and all the larger animals, from the face of the earth.

If there were no proviſion in nature to check the increaſe of ſmall plants and animals, the larger would ſpeedily be deſtroyed by them.

Though all old perſons who have concerned themſelves in agriculture remember the blight in corn many years, yet ſome have ſuppoſed that of late years it has materially increaſed; this however does not ſeem to be the caſe. Tull, in his Horſe-hoeing Huſbandry, p. 74, tells us, that the year 1725 "was a "year of blight the like of which was never before heard of, and "which he hopes may never happen again;" yet the average price of wheat in the year 1726, when the harveſt of 1725 was at market, was only 36*s.* 4*d.* and the average of the five years of which it makes the firſt, 37*s.* 7*d.*—1797 was alſo a year of great blight; the price of wheat in 1798 was 49*s.* 1*d.* and the average of the five years, from 1795 to 1799, 63*s.* 5*d.**

The blight does not appear to have increaſed of late years.

The climate of the Britiſh Iſles is not the only one that is liable to the blight in corn; it happens occaſionally in every part of Europe, and probably in all countries where corn is grown.

The blight in corn probably happens in all countries.

* The ſcarcity of the year 1801 was in part occaſioned by a mildew which, in many places, attacked the plants of wheat on the S.E. ſide only: but it was principally owing to the very wet harveſt of 1800. The deficiency of wheat, at that harveſt, was found, on a very accurate calculation, ſomewhat to exceed one fourth. But wheat was not the only grain that failed: all others, and potatoes alſo were materially deficient. This year the wheat is probably ſomewhat more damaged than it was in 1800, and barley ſomewhat leſs than an average crop. Every other article of agricultural food is abundant, and potatoes one of the largeſt crops that has been known. But for theſe bleſſings on the labour of man, wheat muſt before this time have reached an exorbitant price.—*Sir J. B.*

Italy is very ſubject to it, and the laſt harveſt of Sicily has been materially hurt by it. Specimens received from the colony of New South Wales ſhew that conſiderable miſchief was done to the wheat crop there in the year 1803 by a paraſitic plant, very ſimilar to the Engliſh one.

Common opinion that wheat in the neighbourhood of barberries is moſt frequently blighted.

It has been long admitted by farmers, though ſcarcely credited by botaniſts, that wheat in the neighbourhood of a barberry buſh ſeldom eſcapes the blight. The village of Rolleſby in Norfolk, where barberries abound, and wheat ſeldom ſucceeds, is called by the opprobrious appellation of Mildew Rolleſby. Some obſerving men have of late attributed this very perplexing effect to the farina of the flowers of the barberry, which is in truth yellow, and reſembles in ſome degree the appearance of the ruſt, or what is preſumed to be the blight in its early ſtate.

The barberry leaf is very ſubject to a mildew fungus;

It is, however, notorious to all botanical obſervers, that the leaves of the barberry are very ſubject to the attack of a yellow paraſitic fungus, larger, but otherwiſe much reſembling the ruſt in corn.

which may be transferred to corn.

Is it not more than poſſible that the paraſitic fungus of the barberry and that of wheat are one and the ſame ſpecies, and that the ſeed transferred from the barberry to the corn is one cauſe of the diſeaſe. Miſletoe, the paraſitic plant with which we are the beſt acquainted, delights moſt to grow on the apple and hawthorn, but it flouriſhes occaſionally on trees widely differing in their nature from both of theſe: in the Home Park, at Windſor, miſletoe may be ſeen in abundance on the lime trees planted there in avenues. If this conjecture is founded, another year will not paſs without its being confirmed by the obſervations of inquiſitive and ſagacious farmers.

Conjectures directed to remedy.

It would be preſumptuous to offer any remedy for a malady, the progreſs of which is ſo little underſtood; conjectures, however, founded on the origin here aſſigned to it, may be hazarded without offence.

Probable fact as to its earlieſt appearance.

It is believed * to begin early in the ſpring, and firſt to appear on the leaves of wheat in the form of ruſt, or orange-coloured powder; at this ſeaſon, the fungus will, in all pro-

* This, though believed, is not dogmatically aſſerted, becauſe Fontana, the beſt writer on the ſubject, aſſerts that the yellow and the dark-coloured blight are different ſpecies of fungi.

bability,

bability, require as many weeks for its progreſs from infancy to puberty as it does days during the heats of autumn; but a very few plants of wheat, thus infected, are quite ſufficient if the fungus is permitted to ripen its ſeed, to ſpread the malady over a field, or indeed over a whole pariſh.

The chocolate-coloured blight is little obſerved till the corn is approaching very nearly to ripeneſs; it appears then in the field in ſpots, which increaſe very rapidly in ſize, and are in calm weather ſomewhat circular, as if the diſeaſe took its origin from a central poſition.

The chocolate coloured blight ſeems to ſpread from central points of the field.

May it not happen then, that the fungus is brought into the field in a few ſtalks of infected ſtraw uncorrupted among the maſs of dung laid in the ground at the time of ſowing? it muſt be confeſſed, however, that the clover lays, on which no dung from the yard was uſed, were as much infected laſt autumn as the manured crops. The immenſe multiplication of the diſeaſe in the laſt ſeaſon, ſeems however to account for this; as the air was no doubt frequently charged with ſeed for miles together, and depoſited it indiſcriminately on all ſorts of crops.

Whence it may have been brought in by infected ſtraw in the manure;

It cannot however be an expenſive precaution to ſearch diligently in ſpring for young plants of wheat infected with the diſeaſe, and carefully to extirpate them, as well as all graſſes, for ſeveral are ſubject to this or a ſimilar malady, which have the appearance of orange-coloured or of black ſtripes on their leaves, or or their ſtraw; and if experience ſhall prove that uncorrupted ſtraw can carry the diſeaſe with it into the field, it will coſt the farmer but little precaution to prevent any mixture of freſh ſtraw from being carried out with his rotten dung to the wheat field.

and may, it is likely, be guarded againſt by extirpating the plants firſt infected.

In a year like the preſent, that offers ſo fair an opportunity, it will be uſeful to obſerve attentively whether cattle in the ſtraw yard thrive better or worſe on blighted than on healthy ſtraw. That blighted ſtraw, retaining on it the fungi that have robbed the corn of its flour, has in it more nutritious matter than clean ſtraw which has yielded a crop of plump grain, cannot be doubted; the queſtion is, whether this nutriment in the form of fungi does, or can be made to agree as well with the ſtomachs of the animals that conſume it, as it would do in that of ſtraw and corn.

Whether blighted ſtraw be more or leſs nutritious than clean ſtraw, for cattle?

It

Though the ſeeds of blighted corn afford little flour, they are each as good for ſowing as the plumpeſt ſamples;

It cannot be improper in this place to remark, that although the ſeeds of wheat are rendered, by the exhauſting power of the fungus, ſo lean and ſhrivelled that ſcarce any flour fit for the manufacture of bread can be obtained by grinding them, theſe very ſeeds will, except, perhaps, in the very worſt caſes *, anſwer the purpoſe of ſeed corn as well as the faireſt and plumpeſt ſample that can be obtained, and, in ſome reſpects better; for as a buſhel of much blighted corn will contain one-third at leaſt more grains in number than a buſhel of plump corn, three buſhels of ſuch corn will go as far in ſowing land, as four buſhels of large grain.

and are much more numerous in an equal meaſure.

The flour in the ſhrivelled grain is amply ſufficient to nouriſh the minute plants till they take root.

The uſe of the flour of corn in furthering the proceſs of vegetation, is to nouriſh the minute plant from the time of its developement till its roots are able to attract food from the manured earth; for this purpoſe one-tenth of the contents of a grain of good wheat is more than ſufficient. The quantity of flour in wheat has been increaſed by culture and management calculated to improve its qualities for the benefit of mankind, in the ſame proportion as the pulp of apples and pears has been increaſed by the ſame means, above what is found on the wildings and crabs in the hedges.

It is a waſteful practice to take the plumpeſt grain for ſeed. The ſmalleſt though unfit for the mill are equally good for this uſe.

It is cuſtomary to ſet aſide or to purchaſe for ſeed corn, the boldeſt and plumpeſt ſamples that can be obtained; that is, thoſe that contain the moſt flour; but this is unneceſſary waſte of human ſubſiſtence; the ſmalleſt grains, ſuch as are ſifted out before the wheat is carried to market, and either conſumed in the farmer's family, or given to his poultry, will be found by experience to anſwer the purpoſe of propagating the ſort from whence they ſprung, as effectually as the largeſt.

Arrangement of the grains in an ear of wheat. The lower ones ſupply the moſt food, but the upper are not inferior as ſeed.

Every ear of wheat is compoſed of a number of cups placed alternately on each ſide of the ſtraw; the lower ones contain, according to circumſtances, three or four grains, nearly equal in ſize, but towards the top of the ear, where the quantity of nutriment is diminiſhed by the more ample ſupply of thoſe cups that are nearer the root, the third or fourth grain in a cup is frequently defrauded of its proportion, and becomes

* 80 grains of the moſt blighted wheat of the laſt year, that could be obtained, were ſown in pots in the hot-houſe; of theſe, ſeventy-two produced healthy plants, a loſs of 10 per cent. only.

ſhrivelled

ſhrivelled and ſmall. Theſe ſmall grains, which are rejected by the miller, becauſe they do not contain flour enough for his purpoſe, have nevertheleſs an ample abundance for all purpoſes of vegetation, and as fully partake of the ſap, (or blood, as we ſhould call it in animals,) of the kind which produced them, as the faireſt and fulleſt grain that can be obtained from the bottoms of the lower cups by the waſteful proceſs of beating the ſheaves.

EXPLANATION OF THE PLATES.

Plate IX.

Fig. 1. A piece of the infected wheat ſtraw—natural ſize: at *a* the leaf ſheath is broken and removed, to ſhew the ſtraw which is not infected under it.

Fig. 2. A highly magnified repreſentation of the paraſitic plant which infects the wheat: *a* in a young ſtate; *b* full grown; *c* are two plants burſting and ſhedding their ſeeds when under water in the microſcope; *d* two plants burſt in a dry ſtate; *e* ſeems to be abortive; *f* ſeeds in a dry ſtate; *g* a ſmall part of the bottom of a pore with ſome of the paraſitic fungi growing upon it.

Fig. 3. A part of the ſtraw of *fig.* 1, magnified.

Fig. 4. Part of *fig.* 3 at *a b* more magnified.

Fig. 5. Part of a ſtraw ſimilar to *fig.* 3, but in its green ſtate, and before the paraſitic plant is quite ripe.

Fig. 6. A ſmall part of the ſame, more magnified.

Plate X.

Fig. 1. A highly magnified tranſverſe cutting of the ſtraw, correſponding with *Fig.* 4. *Plate* I. ſhewing the inſertion of the paraſite in the bark of the ſtraw.

Fig. 2. A longitudinal cutting of the ſame; magnified to the ſame degree.

Fig. 3. A ſmall piece of the epidermis of a ſtraw, ſhewing the large pores which receive the ſeed of the paraſite; the ſmaller ſpots obſervable on the epidermis, are the baſes of hairs

hairs that grow on the plant of the wheat whilſt young, but which fall off when it ripens, magnified to the ſame degree as the preceding figures.

II.

Theorems reſpecting the Properties of the Sides of Triangles interſected by Right Lines drawn from the three Angles ſo as to meet in one Point. By Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

Middlehaw, March 1, 1805.

Certain properties of triangles.

GEOMETRICIANS have paid but little attention to the properties of the ſides of triangles, which are interſected by right lines, drawn from the three oppoſite angles ſo as to meet in one point. Perhaps all the theorems of this kind that have been hitherto publiſhed, are contained in the ſecond book of Emerſon's Geometry, with the ſingle exception of a propoſition given by Mr. Landen; who has proved that if one of the internal angles of a triangle, and the two oppoſite external angles of the ſame be biſected, the right lines, drawn for this purpoſe, will alſo meet in a common point. The following propoſitions take a more general view of the ſubject; and as ſome mathematical papers have appeared of late in the Philoſophical Journal, I have ventured to aſk a place for the preſent ſheet, in your valuable miſcellany.

I remain, &c.

JOHN GOUGH.

P. S. In vol. X. page 66, line 2, for $5\frac{1}{2}$ yards read $3\frac{1}{2}$ yards.

Propoſition 1ſt. Let three right lines C M, B N and A S, (*Plate* XI. *Fig.* 1, 2.) drawn from the three angles of a triangle A B C, interſect the oppoſite ſides, produced or not, in the points M, N and S, and alſo meet each other in a common point O; then if all the points M, N and S lay betwixt the

the angular points B, A and C, the place of O will be within the triangle, and the three lines C O, B O and A O will fall wholly within the ſame; but if one of theſe points, as S, lay in B E, which is C B produced, the place of O will be external to the triangle; in which caſe, one of the two remaining points of interſection will lay in its reſpective ſide produced; but the ſituation of the other will always be betwixt the angular points of the third ſide. Certain properties of triangles.

Demonſtration.

Caſe 1ſt. Let M, N and S (*Fig.* 1.) be betwixt the angular points B, A and C; in which caſe, the triangles A B C, M B C are of the ſame altitude; conſequently they are as their baſes A B, B M, *Euc.* 1.6; but B M is a part of A B by hypotheſis; therefore the triangle M B C is a part of A B C, (*Simpſon's Euclid.* D. 5.) that is, the triangle A B C contains the triangle M B C; for the ſame reaſon, the ſame A B C contains the triangle S A B; but the figure M B S O is common to the triangles M B C, S A B; therefore it is contained in the triangle A B C; conſequently the place of O is within the ſame: now as the points A, B, C and O are all found in the ſpace A B C, the lines A O, B O and C O muſt alſo lay wholly within the ſame triangle.

Caſe 2. When the point S falls in B E or C B produced, (*Fig.* 2.) the angle C B A is external to the triangle A B S; therefore it is greater than the angle B S A, (*Euc.* 17, 1;) and the ſum of the angles C A B, A C B is leſs than the angles C A S, A C S taken together, (*Euc.* 32. 1); hence the angle C A B is leſs than C A S; conſequently the right line A S, produced at pleaſure, falls wholly without the triangle A B C. Now if the remaining two lines C M and B N be ſuppoſed to cut the ſides B A and A C not produced, the place of O, their common interſection, will be in the ſpace A B C, by *Caſe* 1: but this is impoſſible; for O is in the right line A S by hypotheſis; which has been ſhewn to fall wholly without the triangle A B C, conſequently one at leaſt of the lines C M, B N muſt alſo lay without the ſame ſpace; let this be B N, and draw O K parallel to C A; then the external angle O K E of the triangle O K C is greater than the internal and oppoſite angle

Certain properties of triangles.

angle B C M, 17.1; but the angles O K E, A C B are equal (*Euc.* 29.1;) therefore the triangle M B C is a part of the triangle A B C, and is contained in it; but theſe triangles are as their baſes M B, B A (*Euc.* 1.6;) conſequently M B is a part of B A, Simſ. *Euc.* D. 5; and the point M is between the points A and B. Q. E. D.

Corollary. Hence it appears that if two lines C M and B N be drawn, and their interſection O lay within the triangle A B C, the place of the third point S will be in the ſide B C unproduced. But if O lay without the triangle, the place of the third point will depend on the ſituation of the given points; namely if theſe be in two ſides produced, the place ſought will lay in the remaining ſide unproduced; but if either of the given points fall between two angles of the triangle, the third will be found in the remaining ſide produced.

Propoſition 2. If a right line C M (*Fig.* 1.) be drawn from C, one of the angles of a triangle A B C, to cut the oppoſite ſide A B, produced or not, in M, and if from the point M, perpendiculars, M F, M H be let fall upon the ſides A C, C B, the rectangle B M, M F will be to the rectangle A M, M H, as B C is to C A.

Demonſtration. Draw A P, B Q perpendicular to B C, C A; then we have the following proportions, by ſemilar triangles; as M B : M H : : A B : A P; and as M F : M A : : B Q : B A; hence as M B × M F : A M × M H : : B Q : A P. But as B Q : A P : : B C : C A, *Euc.* 14.6; therefore the rectangle M B, M F is to the rectangle M A, M H as B C is to C A. *Euc.* 11.5. Q. E. D.

Cor. 1ſt. If O *h*, O *f* be drawn perpendicular to B C, C A, from any point O in M C; the rectangle M B, O *f* will be to M A, O *h* as B C is to C A; for by ſimilar triangles as *f* O : O *h* : : F M : M H; hence as M B × O *f* : M A × O *h* : : M B × M F : M A × M H; conſequently as M B × O *f* : M A × O *h* : : B C : C A. *Euc.* 11.5.

Cor. 2nd. Let the triangle A B C be iſoſceles, having the ſides B A, A C equal; alſo let C M, B N, drawn to the oppoſite ſides B A, A C meet in O; then the point S is given, in which A O, produced if neceſſary, cuts the remaining ſide C B. For draw O *f*, O *g* perpendicular to C A, A B, and we ſhall have, as B S × O *f* : C S × O *g* : : B A : A C, *Cor.* 1; but

B A,

B A, A C are equal by hypothesis; therefore B S × O*f* = C S × O*g*; hence as O*g* : O*f* :: B S : S C; consequently the point S may be found, proper attention being paid to *Cor. Prop.* 1. Certain properties of triangles.

Proposition 3. If three right lines C M, B N, A S, drawn from the three angles of a triangle A B C (*Fig.* 1 and 2) meet each other in a point O, and the opposite sides B A, A C and C B, in M, N and S; the segments of any one side are as the rectangle under the alternate segments of the remaining two sides; *i. e.* B S is to S C as the rectangle B M, A N to the rectangle C N, A M.

Demonstration. Draw O*f*, O*g*, O*h* perpendicular to C A, A B and B C. Then as B C : C A :: B M × O*f* : A M × O*h*; *Cor.* 1. *Prop.* 2; for the same reason as A B : B C :: N A × O*h* : N C × O*g*; hence as B A : A C :: M B × N A × O*f* : N C × M A × O*g*; but as B A : A C :: B S × O*f* : C S × O*g*; consequently B S is to S C as the rectangle B M, A N is to the rectangle C N, A M. Q. E. D.

Cor. If two right lines C M, B N be drawn from two angles of any plain triangle A B C, so as to meet each other in a point O, and the opposite sides B A, A C in M and N; a point S may be found in the remaining side, produced if necessary, so that if S A be joined, the right line A S shall pass through O. For since the right lines B M, M A, A N and N C are given, the rectangles B M, A N and C N, A M are also given; consequently the ratio of these rectangles is given: but the right line A O cuts the side C B, produced or not, according to circumstances, in the same ratio, by the last proposition; therefore if both M and N lay betwixt the angular points of the triangle, or in their respective sides produced, divide B C in S, so that B S may be to S C in the ratio of the rectangles B M, A N and C N, A M; then will S be the required point by *Cor.* to *Prop.* 1; but if M be situated betwixt the angles B, A, and N lay in C A produced; find S in C B also produced, so that B S may be to S C as the rectangle B M, A N is to the rectangle C N, A M; and S will be the required point, by the corollary last referred to.

Prop. 4. Let three right lines C M, B N and A S (*Fig.* 1 and 2) drawn from the three angles of a triangle A B C, intersect each other in a common point O, and meet the oppo-

site

Certain properties of triangles.

fite fides B A, A C and C B, in the points M, N and S; if A N be to N C as A^pB is to B^pC; but B M be to M A as B^rC is to C^rA; then will B S be to S C as the product of B^pA into B^rC is to the product of B^pC into A^rM.

Demonſtration. As A N : N C :: B^pA : B^pC; and as B M : M A :: B^rC : C^rA by hypotheſis; hence as B M × A N : C N × A M :: $B^pA \times B^rC$: $B^pC \times C^rA$; but as B M × A N : N C × A M :: B S : S C; conſequently B S is to S C as the product of B^pA into B^rC is to the product of B^pC into C^rA. *Euc.* 11. 5. Q. E. D.

Corollary 1ſt. The preceding theorem is general; for the ratios of B M to M A, and of A N to N C can be expreſſed by ſome power or powers of the lines B C, C A and A B, B C, which contain the angles oppoſite to the ſides B A and A C; therefore if the points M, N and S be arranged according to the corollary to Propoſition 1ſt, and the ſegments betwixt theſe points and the adjacent angles have the ratios aſſigned in the preſent theorem, right lines, drawn from M, N and S to the oppoſite angles, will all meet in one point.

Cor. 2. When r and p are equal, B S is to S C as B^pA is to A^pC by the propoſition; therefore if the ſegments betwixt the points M, N, S and the adjacent angles are in the ratios of the adjacent ſides raiſed to any given power, right lines, drawn from M, N and S to the oppoſite angles, will all meet in one point by *Cor. 1ſt.*

Cor. 3rd. Let $p = o$; then as B S : S C :: B^oA : A^oC by the propoſition; but B^oA : A^oC is the ratio of equality; therefore if M, N and S biſect the three ſides of a triangle, lines drawn from them to the oppoſite angles will meet in one point by *Cor. 2nd.*

Cor. 4. Let $p = 1$; alſo let M and N interſect the ſides B A, A C not produced; then S lays between the angular points B, C, *Cor. Prop.* 1; and as B S : S C :: B A : A C, *Cor.* 2; therefore the right line S A biſects the angle B A C; conſequently if three right lines biſect the three internal angles of a triangle, they ſhall all meet in one point by the laſt propoſition.

Cor. 5. Let $p = 1$ as before; alſo ſuppoſe M to be in the ſide B A not produced; but let N be without the triangle; then S is in C B produced, *Cor. Prop.* 1; but as B S : S C ::

B A : A C, *Cor.* 2; therefore S A biſects the angle B A N. (*Simpſon's Euclid.* .6 A); alſo as B M : M A : : B C : C A; therefore C M biſects the angle A C B, *Euc.* 3.6; conſequently if two lines biſect any two of the external angles of triangles, and a third line biſect the internal angle oppoſite to theſe angles, theſe lines ſhall meet in one point by the laſt propoſition.

Prop. 5. If B M be to M A as B^pC to C^pA; and h O, Of be perpendicular to B C and C A; h O is to Of as $B^{p-1}C$ to $C^{p-1}A$.

Demonſtration. As B M × Of : A M × Oh : : B C : C A, *Cor.* 1. *Prop.* 2; and as B^pC : A^pC : : B M : M A by hypotheſis; therefore as $B^pC \times Of$: $A^pC \times Oh$: : B C : C A; conſequently as Oh : Of : : $B^{p-1}C$: $C^{p-1}A$. Q. E. D.

III.

A Communication on the Uſe of Green Vitriol, or Sulphate of Iron, as a Manure; and on the Efficacy of paring and burning depending, partly, on Oxide of Iron. By GEORGE PEARSON, M. D. *Honorary Member of the Board of Agriculture, F. R. S. From a Communication made by him to the Board, and inſerted in the fourth Volume of their Tranſactions.*

(Concluded from page 214.)

APPENDIX.

THE following facts, lately diſcovered by moſt reſpectable chemiſts, appear to be worth adding to the preceding memoir, as they ſerve to ſhew that other ſalts, beſides ſulphate of iron and certain earths, may be employed advantageouſly as manures, although like iron they have been eſteemed deleterious to plants.

1. *Aſhes of Pit Coal are a good Manure for Graſs.*

The aſhes of pit coal are a good manure for graſs.

My much valued friend, the Rev. William Gregor, of Grampound, on examination of the aſhes of coal from Liverpool, found them to contain both ſulphate of magneſia and ſulphate of lime, eſpecially the former, ſalt. I apprehend that theſe aſhes alſo contain oxide of iron, or perhaps ſulphate of

of iron. Theſe aſhes, ſays Mr. Gregor, "*ſkeaded*" * over graſs apparently produced good effects *notwithſtanding the ſulphate of magneſia*, which I was well aſſured they contained. See Nicholſon's Journal, Vol. V. p. 225.

Sulphate of magneſia may not be hurtful to plants, tho' pure magneſia is.

From this obſervation of Mr. Gregor, it ſeems he is aware of the prevailing popular opinion, that ſulphate of magneſia is not favourable to vegetation; and to reconcile his fact with the unfriendly nature of magneſia to plants, as diſcovered by Mr. Tennant, he obſerves that the effects of ſulphate of magneſia may be very different from thoſe of magneſia and carbonate of magneſia. I apprehend it is the magneſia (calcined magneſia) only which this learned chemiſt found hurtful to vegetables, as the diſcovery was made on the examination of Nottingly lime, which the farmers near Doncaſter employ as a manure, while they reject the lime of their own neighbourhood. In the latter Mr. Tennant met with magneſia, and in the former none. See the account of this important diſcovery in the Philoſ. Tranſactions.

2. *The Earth from Aſhes called Cinis, is a durable and efficacious Manure; by Profeſſor Mitchill, of New York, one of the Repreſentatives in Congreſs. Addreſſed to Dr. Pearſon.*

Earth from aſhes is a good manure.

Dr. Mitchill, in a letter addreſſed to me on *cinis*, or earth found in the aſhes of wood, has made ſome obſervations relative to the preceding memoir, which ſeem worthy of notice.

"Aſhes of wood contain very commonly ſulphate of potaſh, alſo phoſphoric acid, beſides other well known ſalts; but after theſe ſalts are ſeparated by lixiviation, there remains a *peculiar earth*, and a ſmall proportion of iron. This earth differs from lime, baryt, magneſia, ſtrontian, or any other known ſpecies of earth. I would call it *cinis*, for plentiful, common, and important as it is, ſcience has not dignified it with a name. To judge of the excellence of this earth as a manure, after all the ſalts are extracted from ſoap boilers' aſhes, the earth ſells for ten cents the buſhel, and notwithſtanding this high price, it is not unuſual for the farmer to pay for the article twelve months beforehand. When ploughed into ſterile ground, at twelve loads per acre, it produces great crops of wheat, clover, and other ſorts of graſs and grain, and its fertilizing operation will laſt twenty years. Although ſome

* From Σκεδαω.

of

of the other ingredients of the aſhes left after lixiviation may prove beneficial; yet the effects are chiefly from the cinis, or new named earth.

"This earth, which is ſo prized in America as a manure, was eſteemed of old in Aſia, as an ingredient in a cement: among the ancient Syrians, it was one of the materials forming the plaſter of their walls; and as it holds an intermediate place between lime and potaſh, it can eaſily be conceived how it may act both as a cement and a manure. It is to be hoped, chemiſts will turn their attention to this important ſubject." See Tilloch's Philoſ. Magazine, Vol. VII. p. 273, for the whole of this intereſting letter.

3. *Several Metallic Salts promote Vegetation, ſhewn by the Experiments of Profeſſor Barton, of Philadelphia.*

Letter from Benjamin Smith Barton, M. D. Profeſſor of Medicine in the College of Philadelphia, to Dr. Pearſon, containing Experiments with Metallic Solutions to determine their Effects on Plants.

SIR,

Philadelphia, Oct. 28, 1802.

IN the *Annals of Medicine* for the year 1801, you inform us, that you have lately read a paper at the Board of Agriculture, "containing an account of the effects of a ſaline body collected from peat, as a moſt powerful manure, which turns out to be ſulphate of iron; a ſubſtance (you remark) hitherto conſidered to be a poiſon to plants." This piece of intelligence gave me much ſatisfaction. I have, for ſome years, been engaged in an extenſive ſeries of experiments, relative to the effects of various ſtimulating articles, ſuch as camphire, &c. upon vegetables; and on the abſorption of certain powerful mineral ſubſtances into the organic ſyſtem of vegetables. In numerous inſtances I have ſubjected the ſtems and leaves of plants, young and old, large and ſmall, to the influence of the ſulphates of iron and copper. I have found, that both of theſe metallic ſalts are very *greedily* abſorbed by vegetables, inſomuch that I have detected the preſence of iron in the veſſels of a branch of mulberry, at the height of five or ſix feet above the place of immerſion in a ſolution of the ſulphate of this metal. A full account of my experiments, I deſign

Direct experiments on the effect of metallic ſalts on vegetation.

to communicate to the public in two memoirs. Permit me to obſerve, in the meanwhile, that the ſulphate of iron, applied to vegetables in the manner I have mentioned, " is only (to uſe your own words) a poiſon, like almoſt every thing elſe, from the over-doſe." *In ſeveral of my experiments, the branches of vegetables that were placed in veſſels containing ſolutions of ſulphate of iron and copper, lived longer and exhibited more ſigns of vigour, than ſimilar branches that were placed in equal quantities of ſimple water.* It is true, that, in many other experiments, theſe metallic ſalts proved fatal to my plants; but this was when I employed too large a doſe. In like manner I had found ſeveral years ago*, that camphire, by greatly ſtimulating, often kills vegetables; and yet, when properly doſed, this is a very wholeſome ſtimulant to plants. I had alſo found, that large doſes of nitre (which is unqueſtionably a powerful ſtimulant both with reſpect to animals and vegetables) produce an appearance like genuine gangrene in the leaves of vegetables: and yet it is certain that nitre, when it is judiciouſly doſed, may be made to greatly aſſiſt the healthy vegetation of plants.

Excuſe the liberty I have taken in troubling you with theſe few looſe hints, and permit me to ſubſcribe myſelf,

Sir, your very humble and obedient Servant, &c.

BENJAMIN SMITH BARTON.

4. *Sulphate of Iron in the Peat of Ruſſia, found by Profeſſor Robiſon.*

Sulphate of iron exiſts in Ruſſian peat.

Something elſe beſides vegetable matter is neceſſary to form peat or black moſs of the moors. The ſmell of burning peat is different from that of vegetable matter. Peat aſhes, ſays the Profeſſor, always contain a very great proportion of iron; he has ſeen three places in Ruſſia where there is ſuperficial peat moſs, and in all of them *the vitriol is ſo abundant as to efflorefce.* In particular, on a moor near St. Peterſburgh, the clods ſhew the vitriol (ſulphate of iron) every morning when the dew has evaporated. According to this learned Profeſſor's obſervation, the ſulphate of iron in pit coal may be accounted for in the following manner: " peat moſſes form very regular

* See Tranſactions of the American Philoſophical Society, Vol. IV. No. xxvii.

ſtrata,

ſtrata, lying indeed on the ſurface; but if any operation of nature ſhould cover this with a deep load of other matter, it would be compreſſed and rendered very ſolid: and remaining for ages in that ſituation, might ripen into a ſubſtance very like pit coal. See the Medical and Chirurgical Review for November 1803.

5. *Mr. Anſtey's Teſtimony of the Uſe of Peat Duſt and Peat Aſhes.*

SIR,

Houghton Regis, Dec. 3, 1801.

I received yours, dated the 18th of November laſt, in which you requeſted me to inform you what experiment I had made from the turf duſt, taken from Tingrith Moor. I have made uſe of the aſhes and duſt near thirty years, and I frequently lay on from eighty to a hundred buſhels per acre. Our land is dry and very thin ſtapled, owing to the chalk rock laying ſo very near the ſurface; it encourages vegetation in moiſt warm weather; but when hot and dry, the reverſe. We never mix any other manure with it. It coſts about four-pence per buſhel, including all expences. Teſtimony of Mr. Anſtey.

We chiefly ſpread it on our ſeed graſs, clover, &c.

I am, Sir, your humble Servant,

JOS. ANSTEY.

IV.

On Spectacles. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

DEAR SIR,

Lynn, February 16, 1805.

IN a former paper, I pointed out a property in ſpectacles, which had been overlooked by the writers on optics; but as ſome of your readers entertain opinions contrary to mine, the ſubject ſeems to require further inveſtigation. Poſition reſpecting ſpectacles,

What I have advanced concerning ſpectacles is, that we ſee better by oblique, than by direct pencils of rays *. viz. that we ſee better by oblique, than by direct rays.

* Philoſophical Journal, Vol. VII. p. 291.

It has been demonſtrated by mathematicians, that an oblique pencil of rays has its focus a little nearer the lens, than a direct pencil; but how much this difference amounts to, at different angles, ſeems not to have attracted their attention. This difference, however, is too conſiderable to be diſregarded, when the rays of light fall very obliquely.

Conſiderations relating to oblique pencils.

For let L L (*Fig.* 3. *Pl.* xi.) repreſent a double convex lens, F its focus, A F its axis, and *a b* a ray parallel to it, which will be refracted to F, after paſſing through the lens. Alſo let *c b* and *d d* be two oblique rays falling upon the lens, at *b* and *d*.

Then the rays near A F falling almoſt perpendicularly upon the ſides of the lens, are leſs bent than the rays *c b*, *d d*, which fall obliquely on both ſides of the lens. "For the more oblique they are, the more they are bent, and turned out of their direct way. And conſequently the oblique rays *c b*, *d d*, will ſooner interſect in G, than thoſe at F." (Emerſon's Optics, page 124.) When the angle *a b c* is very conſiderable, the difference between *x* F, and *y* G, will likewiſe be very conſiderable. And the following ſhort account of my experiments will throw ſome light upon the truth of this propoſition.

Spectacles obliquely placed.

In the ſpectacles that I uſe, the reading diſtance of oblique rays is four inches nearer the glaſſes, than the diſtance at which I can ſee beſt, with the direct rays.

And by experiments made with a lens of 24 inches focal diſtance, and 4 inches in diameter, contracted to $\frac{3}{4}$ of an inch, I found the focal diſtance of an oblique pencil of rays 8 inches ſhorter than the focal diſtance of the direct rays. The axis of the lens, when placed for the oblique rays, made an angle of 26° with a line drawn from its centre, to the candle with which the experiments were made. And the diſtance between the candle and the lens meaſured about 10 feet, when the lens was placed to receive the direct rays.

Cor. 1. When an object is ſeen through ſpectacles, by oblique rays, it appears larger than by direct rays.

For the object is ſeen under the greateſt angle, by thoſe rays that are moſt refracted.

Cor. 2. When an object is viewed by oblique rays through ſpectacles, it is ſeen more diſtinctly with one eye, than with the other.

For

For the rays fall more obliquely upon one of the glaſſes, than upon the other.

Cor. 3. The direct pencils of rays are but ſeldom uſed, by a perſon whoſe ſpectacles are of the longeſt focal diſtance, that will afford him diſtinct viſion.

For he generally looks through thoſe parts of his glaſſes, which are remote from their centres, in conſequence of that motion which is given to the eyes, to view objects in different directions.

I am, Dear Sir,

Your moſt obedient Servant,

EZEKIEL WALKER.

V.

Remarks on certain Paſſages in Dr. Thomſon's Chemiſtry, together with ſome Experiments on Sandarach and Maſtic. In a Letter from Mr. R. MATTHEWS.

To Mr. NICHOLSON.

SIR,

IN a work of ſuch importance as Dr. Thomſon's Chemiſtry, which muſt be ſo frequently referred to by the lovers of that ſcience, the ſlighteſt inaccuracy ought not to paſs unnoticed: the following obſervations therefore, perhaps may be conſidered ſufficiently intereſting to deſerve a place in your valuable Journal.

In Dr. Thomſon's chemiſtry,

In his 4th Vol. page 307, Dr. Thomſon has the following remark: "Hitherto it has been affirmed by all chemiſts both ancient and modern, that the alkalies do not exert any action on the reſins." This remark I admit may be generally true; but although moſt chemiſts have been negligent in aſcertaining the effects of alcalies upon ſubſtances of this nature, I conceive ſome of them were not unacquainted with the power they poſſeſs of diſſolving reſinous bodies.

it is ſtated, that Chemiſts formerly aſſerted, that alcalies do not act upon reſins.

Boerhaave, for inſtance, in his elements of chemiſtry, page 545, expreſsly ſays, that, "both the fixed and volatile alcalies have a diſſolving power, upon animal, vegetable, and mineral ſubſtances, ſo far as theſe contain oils, balſams, gums, *roſins*, or gummy roſins, &c."

The contrary is aſſerted by Boerhaave;

and by Hoffmann.

Hoffmann alſo, by the method he employed in diſſolving amber with cauſtic nitre, evinced that he was acquainted with this property in alcalies. Dr. Thomſon deſcribes Mr. Hatchett as the firſt diſcoverer of the action of alcalies upon reſin. The facts above quoted will leſſen the originality, without diminiſhing the value of Mr. Hatchett's ingenious experiments.

How therefore it can be true that, "all chemiſts both ancient and modern, have affirmed, that the alcalies do not exert any action on the reſins" I cannot eaſily diſcern, unleſs indeed we impute to them the moſt palpable contradictions, an error which I believe is not often found in their writings.

Dr. Thomſon aſſerts, that one fifth of ſandarach is inſoluble in alcohol.

Page 340 in the ſame volume, Dr. Thomſon aſſerts, that ſandarach is not ſoluble in alcohol, about a fifth part of it remaining undiſſolved; and deſcribes the inſoluble part as poſſeſſing peculiar properties, differing from ſandarach, and calls it *ſandaracha*. It ought not to be concluded, that Dr. Thomſon himſelf can have chemically examined every ſubſtance he muſt have occaſion to deſcribe in a work which embraces ſo great a variety; but the importance of ſandarach as an ingredient in varniſhes being nearly equal to that of any other reſin, this ſubſtance certainly deſerves more attention from chemiſts than has hitherto been beſtowed on it. The properties of this reſin as mentioned by Dr. Thomſon, are ſtated to reſt ſolely on the authority of Gieſe, no other chemiſt having examined it.

Experiment. It proved ſoluble in eight times its weight of alcohol.

Upon trial I found that ſandarach was ſoluble in eight times its weight of alcohol, a very minute proportion only being left, which appeared to conſiſt almoſt entirely of extraneous matter. Conſequently the ſubſtance Dr. Thomſon has denominated *ſandaracha* can have no exiſtence in this reſin. It is proper to obſerve, that the reſin I made uſe of was ſelected fine, and is known in commerce by the names of gum juniper and ſandarach. From the obvious diſagreement in the reſults, perhaps we ought to conclude, that the ſubſtance employed by Gieſe in his experiments was not true gum ſandarach *.

* Looking to ſome chemical notes, I find that when three or four tears of ſandarach were put into pure alcohol, and left for a day, a ſmall quantity of thick fluid remained at the bottom, which, however, was taken up by agitation, and the whole became ſemi-opake:—It is remarkable, that this ſubſtance is not ſoluble in tallow or oil, as common reſin is. N.

Speaking of maſtich, vol. 3, p. 311. Dr. Thomſon ſays, it is readily ſoluble in alcohol. In the ſame valuable volume, page 332, he informs us, that "that part of maſtich which is inſoluble in alcohol is ſaid to be pure caoutchouc." In inveſtigating this apparent contradiction, I examined the action of alcohol upon maſtich, and found that nearly a fifth part of it remained undiſſolved. This reſiduum, after repeated waſhings in alcohol, was white, conſiderably elaſtic, and adheſive; it was inflammable; and when heated became brown, emitting inflammable gas. In this ſtate it had much the appearance of common Indian rubber, but ſomewhat glutinous: It was not in the leaſt acted on by water.

Dr. Thomſon's account of maſtich.

The Author's experiments.

Alcohol leaves one fifth of maſtich undiſſolved, reſembling Indian rubber.

I was induced to try the effects of different menſtrua on this reſiduum, and on the elaſtic gum caoutchouc, with a view to aſcertain their identity or otherwiſe. The following are the reſults I obtained:

Comparative experiments on the reſidue of maſtich, and on caoutchouc,

Sulphuric ether, previouſly waſhed with diſtilled water, diſſolved this reſiduum as well as caoutchouc.

Alcohol precipitated both theſe ſubſtances from their ſolvent in the form of a white curd.

Water had no action on the ſolutions.

By the nitric acid the reſiduum was converted into a yellow brittle porous maſs, nitrous gas being diſengaged from the acid.

Nitric acid did not act ſo eaſily on caoutchouc; but when raiſed to the boiling point it changed it into a ſimilar ſubſtance.

With the ſulphuric acid a ſubſtance like charcoal was formed, the acid aſſuming a dark port wine colour, part of it being converted into ſulphureous acid.

The ſame effect was produced by the ſulphuric acid on caoutchouc, though not without the aſſiſtance of heat.

Neither the muriatic or oxymuriatic acids appeared to have any action on either of theſe ſubſtances.

Acetic acid diſſolved a ſmall portion of the reſiduum, but had no action on caoutchouc.

Solutions of potaſh and ammonia produced no effect on either the reſiduum or caoutchouc.

From the above experiments it will be obſerved, that the acids act with greater facility on the inſoluble part of maſtich, than on caoutchouc. Notwithſtanding this difference in their action,

Theſe appear to be the ſame.

action, I think it may be inferred, from the coincidence in their effects, that theſe bodies are ſimilar ſubſtances. Probably the action of the acids on the reſiduum was aſſiſted by a ſmall portion of reſin which it was combined with, and which it defended from the action of the alcohol employed to diſſolve the maſtich.

I am, your obedient Servant,

R. MATTHEWS.

March 13, 1805.

VI.

The Dutch Method of curing Herrings, extracted and tranſlated from the German of Krünitz's Economical Encyclopædia (Oeconomiſche Encyclopädie), *Article Häring, by* J. HINCKLEY, *Eſq. F. S. A.*

(Concluded from p. 224.)

Dutch method of catching and curing herrings.

HERRINGS, however, are equally good, wherever they are taken, provided they be but caught in the proper ſeaſon, and well managed. As they die immediately on quitting their element, ſalting and packing are the circumſtances which principally affect their quality. The ſuperior excellence and flavour of Dutch herrings, above thoſe of all other countries, ariſes from the cloſe attention and indefatigable induſtry employed. Every thing, however minute, both as to ſeaſon and management, which can maintain the reputation they have enjoyed for more than two hundred years, is moſt punctually obſerved; and above thirty ordinances on the curing and management of herrings are recited by Sir William Temple, in the 17th century.

Herrings, cured with Scotch ſalt, very quickly decay. Thoſe of Norway are cured in the ſame manner as the Scotch, but with French ſalt, and packed in fir or deal; in conſequence of which they are worſe, and leſs palatable, as they leave a ſour taſte in the mouth, and ſoon ſpoil. In like manner, other nations are equally careleſs in aſſorting the fiſh, and to this may be attributed the precedency which the Dutch herrings have ſo long maintained. The Dutch catch their fiſh regularly and early off Hittland, from the 25th of June, becauſe

becauſe they are then, and to the beginning of July, fatteſt; after which time, the nearer they approach the coaſt, the leaner and worſe they are, Dutch method of catching and curing herrings.

It redounds no leſs to the honour of the Dutch, than to their advantage, that they pay the utmoſt poſſible attention to theſe rules: according to which, the fiſh muſt be taken at the proper ſeaſon, properly ſalted, well aſſorted, and rightly packed; to do all which, the captain and ſailors are by ſeveral laws obliged to bind themſelves by oath, before they ſail. There are alſo overſeers well paid, that they may not betray their truſt, but watch and enforce every the minuteſt regulation; to which circumſtance alſo, the pre-eminence of Dutch herrings throughout the world may partly be aſcribed.

As ſoon as the herrings are taken out of the water, they are thrown either upon the end of the deck, which has been cleared, and made perfectly clean for the purpoſe, or into baſkets; and then *gipped* (the gills and guts taken out) with a knife, by ſome of the crew, who are ſolely employed therein, having been brought up to that practice. The milt, or roe, however, is always left in the fiſh.* What are taken during one night, are, before the following ſun-ſet, neatly and ſkilfully laid in oaken barrels, coarſe Spaniſh or Portugueſe bay ſalt being ſtrewed between. This the fiſhermen of other countries either entirely neglect, or leſs carefully perform, being leſs ſcrupulouſly nice; becauſe they either go out to ſea later, or, like the Scotch, commence fiſhing too ſoon; or only navigate ſmall boats near the coaſt; do not kill the fiſh with a knife, or gut, ſalt, or pack them, while on board, but throw them down in a boat, and when fully laden, go on ſhore, proceed at their leiſure, caſt the fiſh on the ſea-coaſt in conſiderable heaps, where they are even ſuffered to lie ſometimes ſeveral days, before they are gutted, ſalted, and packed, in conſequence of which they grow ſtale and ferment. The Dutch, on the contrary, indefatigably purſue their method day and night, during twenty, twenty-four, or twenty-ſix weeks, be the weather what it may. Hence, their fiſh are uſually more tender, better flavoured, and not ſo very ſalt as the Engliſh and the Scotch.

* Mr. M'Culloch's Treatiſe (ſee note, page 413) ſays, they ſhould alſo be kept cleanly, and out of the ſun, as well as froſt or rain.

There

Dutch method of catching and curing herrings.

There are two methods of ſalting and preſerving herrings for a conſiderable length of time. The one is called white ſalting, the other red. The former is thus performed. Immediately on being taken, the fiſh are gutted, as above deſcribed, and waſhed in clean water. Then ſalt is ſprinkled on them, either internally, or both within and without, and the fiſh, being thrown into large baſkets with handles, are well rouſed, (or ſhaken about a few times) that the ſalt may the better diffuſe itſelf, and penetrate: or, laſtly, which is the beſt method, they are thrown into a tub filled with a ſtrong brine made with bay ſalt and freſh water, in which an egg will ſwim. In this pickle they are left upon deck in the open air, provided the weather be good, during twelve or fifteen hours; but, if circumſtances require, a good deal longer, and are well ſtirred (eſpecially if pickled on ſhore) with ſhovels ſeveral times, that the ſalt may the better and more generally penetrate every where. Laſtly, to pack them properly, they are taken out of the pickle, ſuffered to drain ſufficiently, and then packed in barrels, which are ſtrewed at bottom pretty thickly with ſalt, and, if there is time enough, they are neatly laid in ſtrata or layers, always ſtrewing ſufficient ſalt upon each layer. But, if the abundance of fiſh be too great, they are thrown in promiſcuouſly, with as much ſalt as is requiſite to preſerve them from ſpoiling. When this is done in ſtrata, each new layer is preſſed down hard upon the preceding. This laſt-mentioned proceſs, however, cannot eaſily be attended to at ſea, eſpecially when the fiſhery is very abundant. Hence, if the fiſh are to be exported, or remain long unuſed, they are re-packed on ſhore, laid in freſh ſalt, and preſſed down hard; without which precaution herrings exported by ſea would ſpoil. The ſame practice is purſued in France, Hamburgh, and doubtleſs elſewhere. At Hamburgh, as in Holland, they are packed in the open air, ten packers and three overſeers being appointed for the purpoſe, and all ſworn. Beſides re-packing, the packers in the maritime towns have alſo to pick and aſſort the fiſh, according to their goodneſs, ſalt them anew, and put them in freſh pickle. It is alſo generally a part of the magiſtrates and trading companies oaths, not to ſuffer any bad fiſh to be delivered from the quay or cuſtom-houſe.

After

Dutch method of catching and curing herrings.

After packing, whether the goods are intended for exportation or home-conſumption, whether preſſed down hard or not, they are regularly coopered, that the pickle may not leak out, and the fiſh turn yellow or ſpoil, which takes place the moment they are deficient in pickle. Properly, the herrings ſhould, on the very day on which they are taken, not only be gutted, but ſalted and put in caſks, or at leaſt ſhould not lie more than one night in the firſt pickle. And accordingly ſuch herrings are diſtinguiſhed in France by the name of *Harengs d'une nuit.* But, when the fiſhery is abundant, this is not always poſſible; ſo that only a part can be properly attended to; and the reſt, after being gutted, muſt unavoidably remain, at leaſt the whole following day, if not longer, in the firſt pickle, the regular packing of them being poſtponed till the third day. Theſe fiſh, having ſtood two nights on deck in the open air, are called *Harengs de deux nuits.* But ſuch goods not only are inferior, but do not keep ſo well as the former.

If the fiſhermen mean again to caſt their nets on the following night, or if, on account of the great abundance of the ſhoals, they do not expect to complete the ſalting and regular packing in two days, the fiſh, which they cannot ſo complete, are ſalted in large heaps, and are then called *Slabbers*, or *Slabbegut*, coarſe goods. Theſe are frequently too ſalt, becauſe want of time prevents their being properly managed. They are put into the ſchuyts, which always accompany the herring-buſſes, and waſhed; after which they are ſmoked; though not ſo much as the *Bicklings* (Bücklinge) or red-herrings. The ſalted herrings, hitherto ſpoken of, are called *Böckel herringe*, or pickled herrings, or, in general, plainly *Herrings*; thoſe properly ſalted and packed in layers, *Packed*, or *Barrel herrings;* and thoſe half ſalted, and promiſcuouſly packed in barrels, *Wrack herrings.*

The other mode of curing, called Red ſalting, is thus performed. When the fiſh are taken out of the above-deſcribed pickle, in which, however, they muſt remain longer than thoſe intended for the common, or white-ſalting, and at leaſt four-and-twenty hours; they are hung by the head in rows on wooden poles, in ſtoves conſtructed for the purpoſe, each of which generally contains 12,000 herrings. Being thus placed, a fire is made under them with vine-ſtalks, or any

green

Dutch method of catching and curing herrings.

green faggot-wood, that affords much ſmoke and little flame.* Here they remain till dried, and properly ſmoked, which generally requires twenty-four hours. Thus they become Bicklings, or Red-herrings; when theſe are packed in barrels, they are called Barrelled Bicklings, and are much ſalted; but, if laid in ſtraw, they are called Straw Bicklings, and are ſomewhat leſs ſalted. The excellence of the Bicklings principally conſiſts in their being large, fat, tender, freſh, properly ſalted, pliable, ſoft, of the colour of gold, and not torn or mangled. In Holland, the beſt fiſh are choſen for this purpoſe; but in other places, the above-deſcribed Slabbers only are uſed, or other inferior herrings, deemed unfit for the uſual mode of ſalting. The beſt and fatteſt ſmoked Dutch herrings are called, in German, *Speckbucklinge*, or fat red-herrings; in low Saxon, *Flickhäringe*; and in Hamburgh, *Fläkheeringe*. They are cut open along the back.†

* At Bremen, the place moſt celebrated for ſmoking fiſh, and where no ſecret is made, as in Holland, of the proceſs, they are hung in ovens of the ſize of a ſmall parlour, and ſtrict attention is paid not to uſe fir, or any wood, in which is any the leaſt turpentine or reſinous matter, which invariably gives a bad taſte to the fiſh.

† The act of parliament regulations, and many uſeful obſervations, may be ſeen in two ſmall tracts, 8vo, in poſſeſſion of the Society for the Encouragement of Arts, Manufactures, and Commerce, the one entitled, "Obſervations on the Herring-Fiſhery upon the North and Eaſt Coaſts of Scotland, &c.; with plain Rules, propoſed for curing, and for ſupplying the London Market with White Herrings: By Lewis M'Culloch, many Years employed in furniſhing the Merchants of London with Herrings for Exportation. London. Richardſon, 1788." The other entitled, "The beſt and moſt approved Method of curing White Herrings, and all Kinds of White Fiſh; containing particular Directions how to ſlit, gut, ſalt, dry, and barrel them, fit for Sale at home or foreign Markets; with Directions for boiling of Oil: By a Trader in Fiſh. London. J. Davidſon, 1750."

The

VII.

Th Denſity of Mercury in its ſolid State aſcertained. By Mr. JOHN BIDDLE.

To Mr. NICHOLSON.

SIR, *Birmingham, March* 4, 1805.

AT the Philoſophical Society in Birmingham, of which I am a member, I read a few weeks ago a lecture on mercury. In the courſe of my experiments on this ſubject, conducted with a view of exhibiting this metal in a ſolid ſtate, by expoſure to the frigorific mixture of muriate of lime and ſnow, it appeared to me the ſpecific gravity of this metal would be moſt accurately obtained in compariſon with other metals under ſimilar circumſtances of ſolidity and temperature. I therefore undertook, amongſt a variety of others, the following experiments, to aſcertain its ſpecific gravity in a ſolid ſtate, which I believe had never before been accompliſhed: They ſucceeded to my ſatisfaction, and to the gratification of ſome who were preſent: I am, therefore, inclined to believe, you will think them worth inſerting in your Philoſophical Journal.

Experiments before the Philoſophical Society in Birmingham.

I am very reſpectfully yours,

JOHN BIDDLE.

HAVING purified mercury, by diſtilling from an earthen retort into a glaſs receiver 50 *per cent.* of the quantity put in, to avoid the alloy of any other metal, and afterwards having heated it to 300° of Fahrenheit, to drive off any water that might adhere, ſuch as was uſed in other experiments, and expoſed to the action of muriate of lime and ſnow,—I proceeded as follows:

Mercury was purified by diſtillation;

One thouſand grains of this mercury were introduced to the freezing-mixture, together with three ounces of alcohol, in a round-bottomed glaſs; and having placed in the mercury a fine bent wire, the weight of which was previouſly known when immerſed in the ſame alcohol, to a certain point, in the temperature of 47 degrees above zero of Fahrenheit: this wire, during the congelation of the mercury, was fixed in it, ſo that

and expoſed to congeal under alcohol.

that the whole became attached to the inner ſurface of the glaſs, and could not be ſeparated until the glaſs with its contents was taken out of the freezing-mixture, and warmed, by dipping it in water, ſo as to ſoften the mercury at the ſurface attached to the glaſs; one hand being applied to the wire at the ſame time, to draw the mercury from the glaſs at the moment of its being looſened: it was then replaced, as before, in the freezing-mixture.

Viſible ſigns of great contraction of the mercury by freezing.

During the congelation of the mercury it was obſerved, the ſurface of the metal towards the centre was very conſiderably depreſſed by the contraction of its particles, and the veſſel having been moved during its congelation, a ſmall hole nearly reaching the bottom of the glaſs, was obſerved gradually leſſening in its dimenſions, preſenting a conical cavity with the apex downwards. The wire which held the mercury attached to it was now fixed by the other bent end to the hydroſtatic balance, which held in the oppoſite ſcale the weights by which the mercury had been weighed in the air, together with the balance of the wire immerſed in the ſame alcohol to a point marked on the wire.

Loſs of weight by the immerſion.

It appeared by weighing the 1000 grains of mercury thus immerſed in alcohol, the loſs of weight was 59,8 grains: this was weighed five or ſix times, with the glaſs ſtill in the freezing mixture, with the ſame reſult; but when it was withdrawn only a little way from the muriate of lime and ſnow, the difference of weight was perceptible, in conſequence of the temperature of the mercury and alcohol increaſing.

Comparative experiment with an equal weight of ſilver.

One thouſand grains of pure ſilver, weighed by the ſame ſcale immerſed in the ſame alcohol at the ſame temperature, loſt in weight 88,105 grains: therefore, as the loſs of the weight of the mercury is to the loſs of weight in the ſilver, ſo is the ſpecific gravity of the ſilver to the ſpecific gravity of the mercury.

Whence the ſpecific gravity of ſolid mercury is deduced = 15.612.

The ſpecific gravity of the ſilver in the ſame balance having been aſcertained, by diſtilled water, to be 10,436, it follows, if this ſum be multiplied into the ſum of its loſs ſuſpended and weighed in alcohol, and divided by the ſum of loſs of mercury weighed in the ſame way, that the ſpecific gravity of the mercury in the ſolid ſtate, at about 40 degrees below zero on Fahrenheit's ſcale, equals 15,612.

By

By the ſame hydroſtatic balance it appears, the ſame mercury in a fluid ſtate, the thermometer ſtanding at 47 degrees above zero, is 13,545. Sp. Gr. when fluid, was 13,545.

By theſe experiments it appears, the difference of denſity between its fluid ſtate, at the temperature of 47° above zero, and its ſolid ſtate, at 40° below zero, is 2,0673 in 13545, or 1,5265 in 10, which is 15,265 *per cent.* that is, nearly $\frac{1}{7}$th of its greateſt volume, or nearly $\frac{1}{6}$th of its leaſt volume.

VIII.

Obſervations on Mr. Boswell's *Geometrical Propoſitions. By* An Old Correspondent.

To Mr. NICHOLSON.

SIR, *March* 6, 1805.

When your ingenious correſpondent Mr. Boſwell ſent to you his diſcovery of a ready method of determining a chord-line equal to the ſide of a ſquare of the ſame area as the circle in which it is drawn, it ſhould ſeem that he was not aware that the quantum of error might be aſcertained by calculation, as you have done in the ſubjoined note, otherwiſe he would not have ſatisfied himſelf with the mechanical proof which he has adduced as a teſt of its accuracy. Had it occurred to you, I am perſuaded you would with equal facility have ſhewn, that this gentleman's other "fact in geometry" is ſtill more erroneous, though announced with a degree of confidence which appears to have ariſen from a miſtaken conviction of ſuch obvious accuracy as ſtood not in need of proof. On Mr. Boſwell's lines drawn in a circle.

"*A right line* (B E), ſays he, *drawn from the extremity* B *of the line* I B, *at right angles through the oppoſite diameter* (I F) *to the circumference, will be equal to a fourth of the circumference.*"

Let us try how far this aſſertion is founded in truth. But before we enter upon the examination, I will beg leave to point out a circumſtance in your note, page 152 in your laſt number, which, having ariſen out of inadvertence in your tranſcribing Overſight by inverſion of terms in W. N.'s note.

tranſcribing it moſt probably, may tend to puzzle your readers, and among the reſt perhaps Mr. Boſwell himſelf, who muſt be the moſt intereſted: You have ſaid, "as the radius is to I W, ſo is the diameter to I B," (Plate VIII. Fig. 10); but I take it for granted you meant, "*as* I W *is to radius, ſo is the diameter to* I B," becauſe this analogy is agreeable to the reſult of your calculation. And as you have not mentioned how the doctrine of ſimilar triangles applies in the conſtruction of the figure, I hope you and your numerous readers will not deem me preſumptuous, if I introduce the diagram again with ſome additions, particularly as it will give me an opportunity of proving more intelligibly the error of the ſecond propoſition already quoted. Any triangle formed in a ſemicircle, of which the diameter is one ſide, with its oppoſite angle at the circumference, is a right angled triangle (Eucl. III. Prop. xxxi.): This will be the caſe in Mr. Boſwell's figure if a right line be drawn from B to F, the right angle being at B; and as the angle at I is common to the ſmall right angled triangle I O W, and to the large one I B F, the two triangles are ſimilar, and therefore the hypothenuſe I W of the ſmall one is to its baſe I O, as the hypothenuſe I F (or diameter) is to the baſe I B of the large one. Q. E. D.

Farther elucidation of the note.

How far Mr. B.'s ſecond propoſition is accurate.

With reſpect to the ſecond propoſition, it is very well known (Eucl. III. Prop. xx.) that the angle formed at the centre of a circle is double of the angle at the circumference, if they have the ſame common chord as a baſe; now in the triangle I O W (Plate XI. Fig. 4), the ſides I O being given = .5, O W = .25, and therefore I W = .55901, we have, by a ſimple caſe in plane trigonometry, the angle at I = 26° 33′ 5″.4, the double of which is 53° 6′ 10″.86 for the angle B O F at the centre; but B G, one half of the line B E in queſtion, is the ſine of this angle, and in natural numbers is, by the tables, equal to .7997163, when unity is radius; but, in our figure, the diameter repreſents unity, therefore the double ſine, or chord line, B E is = .7997163, which, according to Mr. Boſwell's aſſertion, ought to be = .7854, or one-fourth of the circumference 3.1416; hence the error is .0143163, or very nearly $\frac{1}{70}$th of the whole.

Apology to theſe remarks.

As I have not the pleaſure of knowing Mr. Boſwell, notwithſtanding I have admired ſeveral ſpecimens of his ingenuity, and as I profeſs to be a promoter of all arithmetical approximations

proximations and ſcientific projects, that have for their means facility of application, and utility for their object, I mean ſo far as they have accuracy to recommend them, I truſt he will not impute any motive of a perſonal nature to me, but give me credit for the declaration, when I aſſert, that the implied invitation collected from his memoir, to aſcertain the accuracy of any of his projects, has been my only inducement for laying before the public the preſent paper.

I am, Sir,

With due reſpect,

AN OLD CORRESPONDENT.

IX.

A Memoir on Milk and the Lactic Acid. By Cit. BOUILLON LAGRANGE.

(Concluded from page 144.)

THE muriate of ſoda, and more eſpecially the muriate of lime, affords a proof of this. Theſe ſalts effect a ſeparation of the curd only in part; and, nevertheleſs, the muriate of lime has a very great affinity for water: the decompoſition ought therefore, in this caſe, to be made in a more ſtriking manner than with the acids.

Curd is not ſeparated by the mere abſtraction of water.

Whey is not therefore the product of fermentation. The alcohol obtained from milk does not prove that this formation is neceſſary in order to obtain the lactic acid; for this laſt may be obtained from a ſerum recently prepared.

Whey not a product of fermentation.

With regard to the formation of the carbonic acid indicated in the ſixth experiment, it is owing to the decompoſition of a ſmall portion of animal matter, and part of the ſugar of milk diſſolved in the ſerum. The more of theſe ſubſtances is decompoſed the greater will be the quantity of carbonic acid formed, and accordingly the degree of acidity is more marked on account of the preſence of the carbonic acid; but if the acid be expelled, the ſerum is milder, and not ſo ſour as that which has been for ſome time expoſed to the air.

Carbonic acid in whey.

If the ſerum be expoſed to the air for ten or twelve days, in proportion as the ſaccharine matter is decompoſed the whey

Acetic acid,

becomes more acid. Theſe degrees of acidity are owing to a ſmall quantity of acetic acid, formed by the aid of carbonic acid and alcohol remaining in the liquor, which muſt naturally augment the acidity of the acid already formed in the milk.

Whether the ſalts in whey render it ſour.

With regard to the ſecond queſtion, Whether whey exhibits acid properties only on account of the ſalts it holds in ſolution?

I have proved that the acid may be ſeparated from the ſaline matters without expoſing the milk to the air for a certain time, as Scheele has directed; and the experiments I ſhall relate in this memoir will afford additional proofs. So that its acid properties do not depend merely upon the ſalts it holds in ſolution, and the lactic acid is not produced by a fermentation ſimilar to the acetous.

Zoonic acid by diſtilling milk;

Though the action of caloric upon milk is generally known, I ſhall nevertheleſs make an obſervation upon the acid obtained by diſtillation with a naked fire, which has for ſeveral years been known by the name of the Zoonic Acid.

is acetous acid.

If this acid be purified and combined with potaſh, a ſalt is obtained, which preſents all the characters of the acetate of potaſh.

Curd contains phoſphate of lime.

I muſt alſo obſerve the cheeſy matter in which ſome chemiſts have been unable to find phoſphate of lime, as announced by Scheele. To obtain this, nothing more is neceſſary than ſtrongly to heat this ſubſtance in a crucible after having well waſhed it, and to calcine it to whiteneſs.

The reſidue is a hard white matter, of which ſome ſmall pieces, veined with blue, may be compared to the turquois.

The nitric acid diſſolves it without effervеſcence; lime-water and ammonia occaſion a flocculent precipitate of phoſphate of lime.

The oxalate of ammonia alſo demonſtrates the preſence of lime.

SECTION III.

Concerning the Serum of Milk.

Examination of whey ſeparated by different agents.

Being deſirous of knowing whether there is any difference between the wheys obtained from milk by different ſubſtances, I made uſe of the mineral acids, ſome vegetable acids, ſalts, ſuch as alum, the acidulous tartrite of potaſh, &c. The coagulating ſubſtance was employed in due quantity; as I had

remarked, that when an exceſs of acid is uſed, it is found to exiſt in the ſerum; whereas, when the neceſſary quantity is uſed, it is found in the cheeſe only.

After having ſeparated the cheeſy matter the ſerum was clarified; the wheys were found to have all the ſame taſte and colour, and poſſeſſed the ſame degree of acidity. It is the ſame fluid in the different caſes.

This freſh whey turns ſyrup of violets green: This property muſt be attributed only to the union of the yellow and blue, and not to ſaline matters, as chemiſts have aſſerted. Whey, which has loſt its colour by expoſure to the air, reddens the ſyrup of violets. It affects ſyrup of violets.

The means employed to aſcertain the acids which had been uſed to coagulate the milk, did not exhibit even the ſmalleſt quantity when the operation was performed with exactneſs.

I made a comparative examination of the ſerum which had become ſour in the open air; and I obtained, by expoſing the milk, the ſame phenomena as were pointed out in the ſecond ſection and third experiment. Sour ſerum by expoſure of milk

The ſpontaneous ſerum differed, 1. In its taſte, which was more acid; and, 2. Its colour, which is dull and whitiſh, owing to white filaments which are ſeparated. It may be had clear by filtration, without acquiring the colour of freſh whey. differed from other ſerum.

The cheeſy matter reſulting from the ſeveral experiments before related, gave the ſame red with tincture of turnſole. If it be calcined and treated with the known re-agents, the preſence of ſulphuric acid will be indicated if this acid has been employed in the coagulation, and alumine when alum has been made uſe of. Cheeſy matter.

I do not therefore ſee any inconvenience, in preparing whey for the purpoſes of pharmacy, to coagulate the milk with one or the other of theſe ſubſtances, more eſpecially if the precautions I have mentioned be attended to. Thoſe who have thought it unwholeſome to coagulate milk with alum, have doubtleſs made no experiment on the ſubject. Alum or ſulphuric acid may be employed to coagulate milk.

Oxigen gas is not abſorbed by the ſerum; even agitation does not facilitate its union. Oxigen and ſerum.

The action of lime-water, the ſolution of barytes, ſtrontian, and ſome metallic ſalts, though already known by chemiſts, require to be again examined, and will enable me to explain the proceſſes for obtaining lactic acid. Other agents.

SECTION IV.

On the Sugar of Milk.

Whether acetic acid in milk ariſe from the decompoſition of its ſugar.

The formation of acetic acid does not ariſe merely from the alteration of the ſugar of milk; and though the experiments already deſcribed admit of certain conjectures, I have aſcertained whether a like quantity of ſugar of milk could be obtained from freſh ſerum and a ſerum expoſed for twelve days at a temperature from 14 to 20 degrees.

A little ſugar is decompoſed by ſtanding.

I have found that ſour milk affords a ſomewhat leſs quantity of ſugar of milk, which proves, that a portion of the ſaccharine matter was employed in forming a quantity of acetic acid.

We may therefore obtain carbonic acid, alcohol, and acetic acid, either by confining whey in cloſe veſſels, or by leaving it expoſed to the air.

Other experiments made on pure ſugar of milk indicated nothing remarkable.

Habitudes of ſugar of milk.

It does not redden tincture of turnſole. When diſſolved in water and expoſed for a long time to the air, it does not become acid. By diſtillation it affords acetic acid along with the known products.

It is ſoluble in weak acetic acid. If a ſmall quantity of freſh gaſeous matter be added, it is ſoon found that the liquor holds it in ſolution. By pouring in a few drops of alkali to ſaturate the exceſs of acid, it loſes its tranſparency, becomes turbid and milky, with a mild taſte reſembling that of milk, and preſerves its opacity for a conſiderable length of time.

General reſults reſpecting milk.

The following reſults are deducible from the preceding facts:—

1. New milk reddens the tincture of turnſole.

2. The cheeſy matter may be ſeparated without contact of the air.

3. The ſerum does not retain the acids made uſe of to coagulate milk.

4. Diſtillation either of milk or of ſugar of milk to dryneſs, affords the acetic acid.

5. The formation of carbonic acid and alcohol is owing to muco-ſaccharine matter.

6. A quantity of acetic acid is formed by the fermentation of theſe ſubſtances.

7. The

7. The cheeſy matter, when ſeparated, always manifeſts the preſence of an acid, and differs in taſte and conſiſtence according to the matters employed in its ſeparation.

8. When waſhed and affording no ſign of acidity, if it be afterwards diffuſed in diſtilled water for ſeveral days, at a temperature between 15 and 22 degrees (about 65 and 80 Fahrenheit), it acquires a ſtrong diſagreeable odour; the fluid ſlightly reddens turnſole, and lime diſengages ammonia.

9. The ſerum and the cheeſy matter contain, beſides the known ſubſtances, phoſphate of lime, as Scheele had announced.

10. The difference which exiſts between freſh ſerum and that which has been expoſed to the air, conſiſts in the acetic acid, which is preſent along with the free acid in the milk.

11. Laſtly, there exiſts in milk and the ſerum a free or uncombined acid, which ſeemed to me to be the acetic acid.

SECTION V.

Concerning the Lactic Acid.

Lactic acid, not obtained by ſimple diſtillation.

Scheele having aſcertained that it is not poſſible to obtain the acid by ſimple diſtillation, had recourſe to peculiar methods to effect this ſeparation.

Scheele's proceſs. Evaporate whey to one-eighth. Filter. Saturate with lime-water, which throws down phoſphate. Filter. Dilute with water. Precipitate lime by oxalic acid. Evaporate to mucous conſiſtence. Diſſolve the acid in alcohol and filter. Then add water, and diſtil off the alcohol. Reſidue is lactic acid.

This chemiſt firſt reduced the whey to one-eighth by evaporation; he then filtered it, and there remained, according to him, no more cheeſy matter.

He ſaturated the fluid with lime-water, and phoſphate of lime was precipitated: This liquor having been filtrated and diluted with three times its quantity of water, he poured in oxalic acid drop by drop, to ſeize and precipitate all the lime; and he aſcertained, by the addition of a little lime-water, that no more oxalic acid remained. He evaporated the liquor to the conſiſtence of honey; the thickened acid was redilſolved in rectified alcohol; the ſugar of milk and all the foreign ſubſtances remained on the filtre, not having been diſſolved by the alcohol. Laſtly, after having again added a little water to the acid held in ſolution by the alcohol, he drew off this laſt fluid by diſtillation, and found the lactic acid in the retort.

It is not pure.

This long and expenſive proceſs does not afford a pure acid. It was therefore neceſſary to adopt a ſimpler proceſs before its nature was examined.

Experiment

With freſh whey little acid is obtained.

Experiment 1. If inſtead of expoſing whey to the air as Scheele directs, the proceſs be immediately performed on the freſh whey, a very ſmall quantity only of acid is obtained, which is leſs coloured and has a more animal ſmell.

This acid appeared to me to be uncombined in the milk, as its quantity was ſo ſmall.

If barytes or ſtrontian and ſulphuric acid be uſed inſtead of the lime-water and oxalic acid of Scheele the reſult is equally perfect. Proceſs with acetate of lead unſucceſsful.

Experiment 2. Inſtead of uſing lime-water and the oxalic acid, I uſed barytes or ſtrontian and the ſulphuric acid. By this means I obtained an acid no leſs pure than that of the Swediſh chemiſt.

Experiment 3. I had likewiſe attempted to decompoſe a lactate of potaſh with acetate of lead, expecting to form a lactate of lead decompoſable by the ſulphuric acid; but the precipitate thus formed is not a lactate of lead, for when a ſufficient quantity of ſulphuric acid is added for the purpoſe of decompoſing it, no uncombined acid is ſet at liberty. It appears that part of the oxide falls down with the animal matter; and this compound is even to a certain degree ſoluble. It is alſo found that the fluid always retains much acetate of lead in ſolution.

We therefore ſee that though this proceſs has been pointed out as ſuperior to that of Scheele, it is certain that it cannot be employed to obtain the lactic acid. The following experiments were made on the lactic acid of Scheele.

Lactic acid of Scheele examined.

1. The intenſity of its colour may be diminiſhed by grinding a piece of red hot charcoal in a mortar, and pouring the lactic acid upon it. A quantity of ſmall flakes were ſeparated which roſe to the ſurface. The fluid was ſlightly boiled and filtered, and the acid was then leſs coloured, and had leſs odour.

2. When this acid is diſtilled in a retort acetic acid paſſes over and a thick yellow very acid matter remains.

3. It may perhaps be thought that part of this acid becomes changed into acetic acid by the aſſiſtance of caloric, as Berthollet obſerves in his Chemical Statics.

"The ternary acids, as this chemiſt obſerves, may be changed into acetic acid, when by the action of heat they abandon part of their carbon, and their elements, which reſiſt this action leſs, are ſeparated by volatilazation."

I have aſcertained that great part of this acid previouſly exiſted in the fluid.

3. The

3. The ſame phenomena take place, but more ſtrikingly, if whey be diſtilled which has been expoſed to the air at the temperature of 15 or 20 degrees (60 and 68 Fahrenheit) for about a fortnight. The firſt product is a clear tranſparent fluid of an acid taſte, and odour of alcohol, which when combined with potaſh, forms an acetate.

If alcohol be added to what remained in the retort, it becomes coloured. After decantation and diſtillation, an acid is found in the retort of a peculiar odour, animalized, and reſembling the preceding; but the acid contains a very ſmall quantity of phoſphate of lime.

Theſe experiments confirm the obſervations I have made: 1. That a mixed acid is obtained by the proceſs of Scheele, and 2. That an acid exiſts in milk and in whey recently prepared. It remains to ſhew the nature of that acid. For that purpoſe I prepared the lactic acid according to the proceſs of Scheele with every neceſſary precaution. I even aſcertained the purity of my alcohol, which having been diſtilled from muriate of lime, marked 40 degrees of the aerometer of Reaumur.

It is a mixed acid.

The lactic acid when diſſolved in alcohol was ſubmitted to diſtillation. There paſſed into the receiver an alcohol which reddened the tincture of turnſole.

When diſſolved in alcohol ſome acid comes over with the ſpirit.

When the acid was totally deprived of alcohol the diſtillation was ſtopped. The lactic acid of Scheele remained of a yellow colour and very ſour taſte.

Lactic acid remained.

Experiment 1. I mixed quick lime with this concentrated acid, and a diſengagement of ammonia took place.*

It gave out ammonia by lime.

Experiment 2. If a lactate of potaſh be formed and evaporated by a gentle heat, a brown matter is ſeparated, which is inſoluble in water. This being afterwards heated in a crucible, the ſalt ſwells up and emits an animal odour. The preſence of pruſſic acid was proved by diſſolving the remaining matter in water and adding a ſmall portion of the ſulphate of iron in ſolution.

Lactate of potaſh leaves pruſſic acid by heat.

* I had before aſcertained the preſence of ammonia in this acid. I announced it on the 15th Nivoſe to the Society of Pharmacy. Mr. Vauquelin, who was not acquainted with this fact, informed me at the reading of the firſt part of this memoir that he had alſo aſcertained the preſence of an animal matter in the lactic acid of Scheele.

Experiment

Sulph. acid added to lactic acid disengages acetous acid.

Experiment 3. If sulphuric acid be added to lactic acid, or to a lactate of potash, a disengagement takes place not only of acetic acid, but also of an elastic fluid, which forms a thick cloud when in contact with ammonia.

Distillation.

Experiment 4. When the preceding mixture is distilled in an apparatus proper to receive the gaseous product in water, the distilled water of the bottle preserves its transparency; the smell of acetic acid is very evident; it reddens turnsole and precipitates the muriate of silver.

When a small quantity of ammonia was brought near the tube out of which the gas issued, a very dense cloud was formed.

Fire decomposed the acid of the lactate of potash.

Experiment 5 What was left in the retort was evaporated to dryness. This residue was brown, very acid, and when afterwards heated in a crucible of platina, swelled up and left a coal.

During this operation a small quantity of sulphuric acid was decomposed.

The matter that remained in the retort was dissolved in distilled water. The solution did not redden turnsole, but it gave a brown colour to paper tinged with curcuma. A little sulphuric acid was added to saturation, and after evaporation crystals were obtained by cooling, which exhibited the characters of sulphate of potash.

As the supernatant liquor had a metallic taste, prussiate of lime was poured on it, which produced a bluish tinge.

Lactic acid decomposed by fire leaves potash.

Experiment 6. The lactic acid may also be concentrated to dryness; heated to incineration in a crucible; afterwards dissolved in a distilled water and precipitated by nitrate of silver. A muriate of silver is formed which may be separated by the filter; and the fluid which contains the nitrate is then evaporated and decomposed by heat. A white matter remains, which when dissolved in water, affords with the tartareous acid an acidulous tartrite of potash.

Component parts of Scheele's lactic acid.

Hence I conclude that the lactic acid of Scheele is composed of acetic acid; muriate of potash; a small portion of iron probably dissolved in the acetic acid; and an animal matter.

X.

An Analyſis of the Magnetical Pyrites; with Remarks on ſome of the other Sulphurets of Iron. By CHARLES HATCHETT, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

§ I.

Univerſal diffuſion and importance of ſulphurets of iron.

OF the various metallic ſulphurets which conſtitute one of the grand diviſions of ores, none appear to be ſo univerſally diſperſed throughout the globe, as the ſulphuret of iron, commonly called Martial Pyrites; for the ſpecies and varieties of this are found at all depths, and in all climates and ſoils, whether ancient, or of alluvial and recent formation. It is remarkable alſo, that, under certain circumſtances, this ſulphuret is daily produced in the humid way; an inſtance of which, a few years back, I had the honour, in conjunction with Mr. Wiſeman, to lay before this Society; * and although, in regard to pecuniary value, the pyrites of iron may be conſidered as comparatively inſignificant, yet there is every reaſon to believe, that in the operations of nature, it is a ſubſtance of very conſiderable importance.

§ II.

Among the numerous ſpecies one is remarkable for ſtrong magnetic polarity.

The ſpecies and varieties of martial pyrites, are in general ſo well known, and have been ſo frequently and accurately deſcribed, as to figure, luſtre, colour, and other external characters, that it would be totally ſuperfluous here to give any detailed account of them. One of the ſpecies, however, merits peculiar notice, as poſſeſſing the remarkable property of ſtrong magnetic polarity; and, although it has been deſcribed by modern mineralogiſts,† it does not appear to have been as yet ſubjected to any regular chemical examination; ſo that, whether it be a ſulphuret of iron inherently endowed with the magnetical property, or a ſulphuret in which particles of the ordinary magnetical iron ore are ſimply but minutely interſperſed, has to this time remained undecided.

* Phil. Tranſ. for 1798, p. 567.

† Kirwan, Vol. II. p. 79. Widenmann, p. 792. Emmerling, 2d edit. Tom. II. p. 286. Karſten, p. 48. Brochant, Tome II. p. 232.

This

Its obvious or external properties.

This ſpecies is known by the name of Magnetical Pyrites, and is called by the Germans *Magnet-Kies*, or *Ferrum mineraliſatum magnetico-pyritaceum*.

It is moſt frequently of the colour of bronze, paſſing to a pale cupreous-red.

The luſtre is metallic.

The fracture is unequal, and commonly coarſe-grained, but ſometimes imperfectly conchoidal.

The fragments are amorphous.

The trace is yellowiſh-gray, with ſome metallic luſtre.

It is not very hard; but, when ſtruck with ſteel, ſparks are produced, although with ſome difficulty.

It is brittle, and is eaſily broken.

Where found.

This pyrites has been hitherto found only in ſome parts of Norway, Sileſia, Bavaria, and eſpecially at Geier, Meſſerſdorf, and Breitenbrunn in Saxony; but, having received ſome ſpecimens from the Right Hon. Charles Greville, F. R. S. I was ſtruck with their reſemblance to the pyrites of Breitenbrunn, which happened at that time to be in my poſſeſſion; and, upon trial, I found that they were magnetical, and agreed with the latter in every particular. Their magnetic power was ſuch as ſtrongly to affect a well-poized needle, of about three inches in length; a piece of the pyrites, nearly two inches ſquare, acted upon the needle at the diſtance of four inches.

Degrees of magnetiſm.

The powder (which is blackiſh-gray, with but little metallic luſtre) is immediately taken up by a common magnet; but the pyrites does not act thus on the powder, nor on iron filings, unleſs it has been placed for ſome time between magnetical bars; then indeed it acts powerfully, turns the needle completely round, attracts and takes up iron filings, and ſeems permanently to retain this addition to its original power.

In the ſpecimens which I obtained, the north pole was generally the ſtrongeſt.

This pyrites was found in Wales, about the year 1798, by the Hon. Robert Greville, F. R. S. who ſent the ſpecimens above deſcribed to his brother, the Right Hon. C. Greville, with the following account.

Abundantly found in Caernarvonſhire.

"It is found in great abundance in Caernarvonſhire, near "the baſe of the mountain called Moel Elion, or probably with "more accuracy Moel Ælia, and oppoſite to the mountain "called

" called Mynydd Mawr. Theſe mountains form the entrance " into a little cloſe valley, which leads to Cywellin lake, near " Snowdon, a little beyond the hamlet of Bettws.

" The vein appears to be ſome yards in depth and breadth, " and ſeems to run from north to ſouth, as it is found on " Mynydd Mawr, which is acroſs the narrow valley, and " oppoſite to Moel Ælia."

Mr. R. Greville, in another part of his letter, ſtates that copper ore has been worked in ſeveral of the adjacent places, and that, many years ago, Capt. Williams, of Glan yr Avon, employed ſome miners at the place where this pyrites is found, but the undertaking proved unproductive. Yellow copper ore is certainly in the vicinity; for ſome portions of it were adhering to the ſpecimens which have been mentioned; and I ſhall here obſerve, that the ſtone which accompanies the magnetical pyrites, is a variety of the lapis ollaris or pot-ſtone, of a pale grayiſh-green, containing ſmooth cubic cryſtals of common pyrites.

§ III.

From the appearance of thoſe parts of the magnetical pyrites which have been expoſed to the weather, it ſeems to be liable to oxidizement, but not to vitriolization.

Magnetic pyrites is ſlightly oxidable by the weather.

The ſpecific gravity, at temperature 65° of Fahrenheit, is 4.518.

Sp. gravity.

When expoſed to the blowpipe, it emits a ſulphureous odour, and melts into a globule nearly black, which is attracted by the magnet.

Effect of mere heat by blowpipe:

Five hundred grains, in coarſe powder, were expoſed, in a ſmall earthen retort, to a red heat, during three hours. By this operation, the weight of the powder was very little diminiſhed; neither was there any appearance of ſulphur in the receiver, which however ſmelt ſtrongly of ſulphureous acid.

— in a crucible;

Five hundred grains of the ſame were put into a flat porcelain crucible, which was kept in a red heat, under a muffle, during four hours. The powder then appeared of a dark gray, with a tinge of deep red, and weighed 432.50 grains. The loſs was therefore 67.50 = 13.50 *per cent.* but, upon examining the reſiduum, I found that only part of the ſulphur had been thus ſeparated.

— in the muffle.

The

Partly ſoluble in dilute ſulph. acid.

The magnetical pyrites, when digeſted in dilute ſulphuric acid, is partially diſſolved, with little efferveſcence, although there is a very perceptible odour of ſulphuretted hydrogen.

The ſolution is of a very pale green colour.

Precipitates.

Pure ammonia produced a dark green precipitate, tending to black; and pruſſiate of potaſh formed a very pale blue precipitate, or rather a white precipitate mingled with a ſmall portion of blue. The whole of the latter, however, by expoſure to the air, gradually aſſumed the uſual intenſity of Pruſſian blue; and the blackiſh green precipitate, formed by ammonia, became gradually ochraceous. Theſe effects therefore fully prove, that the iron in the ſolution was, for the greater part, at the minimum of oxidizement, ſo as to form the green ſulphate, and white pruſſiate, of iron; * and, conſequently, that the iron of the magnetical pyrites is either quite, or very nearly, in the ſtate of perfect metal.

Effect of nitric acid.

This pyrites, when treated with nitric acid, of the ſpecific gravity of 1.38, diluted with an equal quantity of water, is at firſt but little affected; but, when heat is applied, it is diſſolved, with much efferveſcence, and diſcharge of nitrous gas; the efferveſcence, however, is by no means ſo violent as when the common pyrites are treated in a ſimilar manner. It is alſo worthy of notice, that if the digeſtion be not of too long duration, a conſiderable quantity of ſulphur, *in ſubſtance*, is ſeparated; whilſt, on the contrary, ſcarcely any can be obtained from the common pyrites, when treated in a ſimilar manner; although I ſhall ſoon have occaſion to prove, that the real quantity of ſulphur is much more conſiderable in the latter than in the former.

Of muriatic acid.

As ſoon as muriatic acid is poured on the powder of the magnetical pyrites, a ſlight efferveſcence is produced, which becomes violently increaſed by the application of heat; a quantity of gas is diſcharged, which, by its odour, by its inflammability, by the colour of the flame, by the depoſition of ſulphur when burned, and by other properties, was proved to be ſulphuretted hydrogen.

During the digeſtion, ſulphur was depoſited, which ſo enveloped a ſmall part of the pyrites, as to protect it from the farther action of the acid.

* Récherches ſur le Bleu de Pruſſe, par M. Prouſt. Annales de Chimie, Tome XXIII. p. 85.

The

The ſolution was of a pale yellowiſh-green colour. With pruſſiate of potaſh it afforded a pale blue precipitate, or rather a white precipitate mixed with blue; and with ammonia it formed a dark blackiſh-green precipitate, which gradually became ochraceous; ſo that theſe effects corroborated the concluſions which were founded on the properties of the ſulphuric ſolution, namely, that the iron contained in the pyrites, is almoſt, if not quite, in the metallic ſtate. Muriatic ſolution.

Other experiments were made; but, as they merely confirm the above obſervations, I ſhall proceed to give an account of the analyſis.

§ IV.

ANALYSIS OF THE MAGNETICAL PYRITES.

A. One hundred grains, reduced to a fine powder, were digeſted with two ounces of muriatic acid, in a glaſs matraſs placed in a ſand bath. The effects already deſcribed took place; and a pale yellowiſh-green ſolution was formed. The reſiduum was then again digeſted with two parts of muriatic acid mixed with one of nitric acid; and a quantity of pure ſulphur was obtained, which, being dried, weighed 14 grains. Analyſis. 100 gr. magnetic pyrites were digeſted in muriatic acid. Partial ſolution. Reſidue treated with nitro-mur. acid. Some ſulphur ſeparated.

B. The acid in which the reſiduum had been digeſted, was added to the firſt muriatic ſolution; ſome nitric acid was alſo poured in, to promote the oxidizement of the iron, and thereby to facilitate the precipitation of it by ammonia, which was added after the liquor had been boiled for a conſiderable time. The precipitate thus obtained was boiled with lixivium of potaſh; it was then edulcorated, dried, made red-hot with wax in a covered porcelain crucible, was completely taken up by a magnet, and, being weighed, amounted to 80 grains. To the acid ſolutions was added ſome nitric acid and the iron was then precip. by ammonia. The precip. was boiled with potaſh and then reduced to black oxide.

C. The lixivium of potaſh was examined by muriate of ammonia, but no alumina was obtained. The potaſh lixivium gave no alumina.

D. To the filtrated liquor from which the iron had been precipitated by ammonia, muriate of barytes was added, until it ceaſed to produce any precipitate; this was then digeſted with ſome very dilute muriatic acid, was collected, waſhed, and, after expoſure to a low red heat for a few minutes in a crucible of platina, weighed 155 grains. If therefore the quantity of ſulphur, converted into ſulphuric acid by the preceding operations, and precipitated by barytes, be calculated according to the accurate experiments of Mr. Chenevix, theſe 155 grains of ſulphate The firſt ſolution liquor was precip. by muriate of barytes. The ſulphate of barytes thus obtained ſhewed the quantity of ſulph. acid.

ſulphate of barytes will denote, nearly, 22.50 of ſulphur; ſo that, with the addition of the 14 grains previouſly obtained in ſubſtance, the total quantity will amount to 36.50.

Eſtimate of the pure iron in the oxide.

E. Moreover, from what has been ſtated it appears, that the iron which was obtained in the form of black oxide, weighed 80 grains; and, by adding theſe 80 grains to the 36.50 of ſulphur, an increaſe of weight is found = 16.50. This was evidently owing to the oxidizement of the iron, which, in the magnetical pyrites, exiſts quite, or very nearly, in the metallic ſtate, but, by the operations of the analyſis, had received this addition. The real quantity of iron muſt, on this account, be eſtimated at 63.50.

Component parts. Sulphur 4. and iron 7.

One hundred grains, therefore, of the magnetical pyrites, yielded,

Sulphur	A. 14 / D. 22.50	36.50 grains.	
Iron	E. =	63.50	
		100.	

Analyſis repeated by acidifying all the ſulphur by nitric acid. Same reſult.

This analyſis was repeated in a ſimilar manner, excepting that the whole was digeſted in nitric acid, until the ſulphur was intirely converted into ſulphuric acid. To the liquor which remained after the ſeparation of the iron by ammonia, muriate of barytes was added, as before, and formed a precipitate which weighed 245 grains. Now, as the ſulphuric acid in ſulphate of barytes is eſtimated by Mr. Chenevix at 23.5 *per cent.* and the ſulphur which is required to form the ſulphuric acid contained in 100 parts of ſulphate of barytes, at 14.5 *, it follows, that 245 grains of dry ſulphate of barytes, contain ſulphuric acid equal, very nearly, to 36 grains of ſulphur; ſo that the two analyſes corroborate each other. The proportion of ſulphur in the magnetical pyrites, may therefore be ſtated at 36.50, or indeed at 37 *per cent.* if ſome ſmall allowance be made for the occaſional preſence of earthy particles; a minute portion of quartz having been found, by the laſt analyſis, after the complete acidification of ſulphur.

The iron in this and in all pyrites, is very nearly in the metallic ſtate.

The increaſe produced, by the operations of the analyſis, in the weight of the iron, aroſe, as I have already remarked, from the addition of oxygen; for the iron, as obtained by the analyſis, was in the ſtate of black oxide; but in this, and

* Tranſactions of the Royal Iriſh Academy, Vol. VIII. p. 240.

indeed

indeed in all pyrites, it undoubtedly exiſts very nearly, or quite, in the ſtate of perfect metal. Now the black oxide of iron, called Protoxide by Dr. Thomſon *, has been proved, by Lavoiſier and Prouſt, to conſiſt of 100 parts of metallic iron combined with 37 of oxygen, thus forming 137 of black oxide; the exact proportion of oxygen is therefore 27 *per cent*, and 80 grains of this oxide muſt contain 21.6 of oxygen. But, in the above analyſes of the magnetical pyrites, the increaſe of weight did not amount to more than 16.5; and we may therefore conclude that, in all probability, a quantity of oxygen = 5.1 was previouſly combined with ſome part, or with the general maſs, of the iron in the pyrites. A ſmall part of the abovementioned increaſe of weight, muſt likewiſe have ariſen from another cauſe; for, although the true proportions of the black oxide of iron are 27 of oxygen and 73 of iron, (ſo that 100 parts of the latter abſorb 37 of the former,) yet, in actual practice, it is difficult to obtain it exactly in this ſtate, and there is commonly a ſmall exceſs of weight: this I have repeatedly obſerved, in many experiments, ſome of which were purpoſely made. When, for inſtance, 100 parts of fine iron wire were diſſolved in muriatic acid, and afterwards precipitated by ammonia, edulcorated, dried, and made red-hot with a ſmall quantity of wax in a covered porcelain crucible, the weight, inſtead of 137, uſually amounted to 139 or 140. The quantity of wax employed, certainly did not afford a ponderable quantity of coal, or other reſiduum; but the real cauſe of the increaſe of weight, appears to be the air, which can ſcarcely be completely excluded, and which, after the wax is burned, combines with the ſuperficial part of the oxide, and converts a portion of it into the red or peroxide; ſo that the ſurface in the crucible appears brown, when compared with the interior.

To this cauſe, therefore, I am inclined alſo to attribute a ſmall part of the increaſe obſerved in the weight of the iron obtained by the preceding analyſes.

§ V.

Comparative analyſis of other pyrites, by acidifying the whole of the ſulphur.

Before I make any obſervations on the nature of the ſulphuret which has been proved to conſtitute the magnetical pyrites,

* Syſtem of Chemiſtry, 2d edition, Vol. I. p. 147.

pyrites, it may be proper to ſtate ſome comparative analyſes which I have made of ſeveral of the common pyrites; and, as the method employed was preciſely the ſame as that which has been deſcribed, all that ſeems to be requiſite, is to give an account of the reſults.

In each analyſis, the whole of the ſulphur was converted into ſulphuric acid, which was precipitated by barytes; and, in the ſelection of the ſpecimens, great attention was paid to take the internal parts of the fragments, and not to make uſe of any which exhibited an appearance of decompoſition, or of extraneous ſubſtances.

The iron was, as before, reduced to the ſtate of black oxide; and the addition of weight in each ſeparate analyſis, correſponded, within a few fractional parts, with the proportion of oxigen requiſite to form into black oxide a given quantity of metallic iron, equal to that which in each pyrites was aſcertained to be the real proportion, by deducting the quantity of ſulphur from the total quantity of each pyrites.

The iron, therefore, in theſe is completely metallic, and as ſuch is ſtated in the following reſults:

The component parts of the common pyrites were, at a medium, 9 ſulphur and 8 iron, with no great variation.

No. 1. Pyrites in the form of dodecaedrons with pentagonal faces. - - Specific gravity 4830.	Sulphur	52.15
	Iron	47.85
		100.
No. 2. Pyrites in the form of ſtriated cubes.	Sulphur	52.50
	Iron	47.50
		100.
No. 3. Pyrites in the form of ſmooth poliſhed cubes, found in the lapis ollaris which accompanies the magnetical pyrites. Specific gravity 4831.	Sulphur	52.70
	Iron	47.30
		100.
No. 4. Radiated pyrites. - - Specific gravity 4698.	Sulphur	53 60
	Iron	46.40
		100.
No. 5. A ſmaller variety of radiated pyrites. Specific gravity 4775.	Sulphur	54.34
	Iron	45.66
		100.

Conſidering the difference in the figure, luſtre, and colour of theſe pyrites, I expected to have found a much greater difference

difference in the proportions of their component ingredients; but, as the reſults are the average of ſeveral experiments, I have not any reaſon to doubt their accuracy.

Regular cryſtals contain rather more iron than other pyrites.

The pyrites cryſtallized in regular figures, ſuch as cubes and dodecaedrons, according to the above analyſes, contain leſs ſulphur, and more iron, than the radiated pyrites, and perhaps than others which are not regularly cryſtallized. This difference, however, is not conſiderable; for the dodecaedral pyrites, which afforded the ſmalleſt quantity of ſulphur of any of the regularly cryſtallized pyrites, yielded 52.15; and the radiated pyrites, No. 5, gave 54.34; the difference, therefore, is only 2.19. So that the mean proportion of ſulphur, in all the pyrites which were examined, is 53.24 *per cent.* and, taking the proportion of ſulphur in the magnetical pyrites at 36.50 or 37, the difference between this and the mean of the common pyrites will be 16.74 or 16.24. The magnetical pyrites, therefore, is quite diſtinct, as a ſulphuret of iron, from the common martial pyrites; and, in the following obſervations I ſhall prove, that a ſulphuret conſiſting of the proportions laſt mentioned, has till now been unknown as a product of nature.

§ VI.

Hiſtory of pyrites.

Although pyrites is one of the moſt common of mineral ſubſtances, yet the diſcovery of its real nature is comparatively of a late date; for it appears that even Agricola (whoſe knowledge of mineral bodies was certainly great, conſidering the ſtate of ſcience in his time) was not acquainted with its characteriſtic ingredient, namely, *iron*. According to Henckel, this was firſt noticed by our countryman Martin Liſter, a member of this learned Society, who ſays, "*Pyrites purus putus ferri metallum eſt.*"

Experiments of Prouſt.

Two ſulphurets of iron: artificial, which is eaſily ſoluble in acids, and natural, leſs ſoluble.

From the time of Henckel, pyrites ſeems little to have attracted the notice of chemiſts, until Mr. Prouſt, the learned profeſſor of chemiſtry at Madrid, publiſhed two memoirs, in which he ſtates, that there are two ſulphurets of iron, the one being artificial, and the other natural. The firſt is the ſulphuret which is formed in laboratories, by adding ſulphur to red-hot iron, or by expoſing both of them to heat in a retort. This is diſtinguiſhed from the ſecond ſulphuret, (which is the common martial pyrites,) by its eaſy ſolubility in acids, eſpecially

in muriatic acid, by the formation of ſulphuretted hidrogen gas during the ſolution of the ſulphuret in the laſt named acid, by its colour, and by its inferior denſity.

Artificial ſulphuret, 6 ſulphur and 10 iron; natural, 9 ſulphur and 10 iron.

According to Mr. Prouſt, the firſt or artificial ſulphuret is compoſed of 60 parts of ſulphur, combined with 100 parts of iron; whilſt the ſecond ſulphuret, or common pyrites, conſiſts of 90 parts of ſulphur and 100 of iron.

The exceſs of ſulphur is eaſily ſeparated from pyrites; but not the other portion.

He moreover obſerves, that the ſulphur of the firſt ſulphuret is difficultly ſeparated; but that the exceſs which is in the ſecond ſulphuret, or common pyrites, is eaſily expelled, and is that portion which is obtained by diſtillation, the reſiduum being then reduced to the ſtate of the firſt ſulphuret *. 100 parts, therefore, of this ſubſtance, are compoſed of 62.50 of iron and 37.50 of ſulphur; and 100 parts of common pyrites are, according to this ſtatement, compoſed of 52.64 of iron and 47.36 of ſulphur.

They are the maxima and minima of the ſulphurets.

Theſe proportions Mr. Prouſt conſiders as the minimum and maximum of the ſulphurets of iron. For the latter he allows ſome variation; but the compoſition of the former he regards as fixed by the invariable law of proportions †; although he obſerves, that *it has not as yet been diſcovered in the mineral kingdom* ‡.

In ſupport of theſe aſſertions, Mr. Prouſt ſtates,

Experiments of Prouſt on this ſubject.

1. That the pyrites found near Soria, when diſtilled in a retort heated to redneſs, afforded nearly 20 *per cent.* of ſulphur.

2. That the reſiduum of the above diſtillation had loſt the external characters and chemical properties of pyrites, and had aſſumed thoſe of the artificial ſulphuret of iron.

* *Journal de Phyſique*, Tome LIII. p. 89, and Tome LIV. p. 89. From pp. 91 and 92 of Tome LIV. it is evident, that the author does not mean to aſſert, that the firſt ſulphuret contains 60 *per cent.* of ſulphur; but that 100 parts of iron are combined with 60 of ſulphur, and form 160 of the ſulphuret. In like manner, when 90 of ſulphur are united with 100 of iron, a ſubſtance analogous to common pyrites is formed, which weighs 190 grains or parts.

See alſo our Journal, I. 109, 269, 253, for tranſlations of theſe excellent memoirs.

† *Journal de Phyſique*, Tome LIII. p. 90.

‡ " La regne minéral, juſqu'ici, ne nous a point encore préſenté le fer ſulphuré au minimum." *Journal de Phyſique*, Tome LIV. p. 93.

3. That when to this reſiduum a quantity of ſulphur was added, and the whole was diſtilled in a degree of heat not too great, the 20 *per cent.* of ſulphur, which had been ſeparated by the firſt diſtillation, was, by this, again reſtored; and the maſs in the retort thus recovered nearly the original colour, luſtre, and chemical properties of the pyrites.

4. That, by adding ſulphur to iron filings, or fine iron wire, heated to a low red in a retort, a compound is obtained, in which the proportion of ſulphur amounts only to about 20 or 30 parts; but, if this compound is again treated with ſulphur in a red heat, a ſulphuret is formed, which is readily diſſolved in acids, and plentifully affords ſulphuretted hydrogen gas.

This is the real minimum of the ſulphurets of iron, fixed by the invariable law of proportions, (according to Mr. Prouſt,) at 59 or 60 of ſulphur and 100 of iron, the former being (as I have already obſerved) in the proportion of 37.50 *per cent.*

5, and laſtly. That when this ſulphuret is again mixed and diſtilled with ſulphur, (due attention being paid to the degree of heat,) the product is found to have aſſumed moſt of the chemical and external properties of the natural common pyrites, denſity alone being excepted.

The application of the above obſervations, to the principal ſubject of the preſent paper, is ſufficiently obvious; for, when it is conſidered, that the magnetical pyrites is ſo different from the common pyrites, in colour, hardneſs, ſolubility in ſulphuric acid, and more eſpecially in muriatic acid, with the copious production of ſulphuretted hydrogen gas; when, by analyſis, it has been found to conſiſt of 36 or 37 of ſulphur, combined with about 63 of metallic iron; and, when the artificial ſulphuret of iron which has been lately deſcribed, is proved to agree with the magnetical pyrites in the nature and proportions of its component ingredients, and in every one of the above-mentioned properties; it is evident that the magnetical pyrites is identically the ſame with this ſulphuret, which hitherto has remained undiſcovered in nature, and has only been known as a product of our laboratories. In order however more fully to ſatisfy myſelf, I made experiments on the artificial ſulphuret, which I formed with ſulphur and fine iron wire.

The magnetical pyrites is that which Prouſt conſidered as having been only produced by art.

The artificial pyrites agrees with the natural magnetic pyrites.

This ſubſtance agreed, in all the properties which have been noticed, with the magnetical pyrites; and the precipitates obtained by adding pruſſiate of potaſh, and ammonia, to the muriatic and ſulphuric ſolutions, were preciſely ſimilar. The ſpecific gravity was 4390, whilſt (as I have already remarked) that of the magnetical pyrites is 4518.

(To be concluded.)

XI.

Anſwer to a Letter of C. L. *with other Remarks, on the Images formed by convex Lenſes. In a Letter from Mr.* Ezekiel Walker.

To Mr. NICHOLSON.

Dear Sir,

Remarks on the Letters of C. L.

C. L. in his letter in your laſt Number, repreſents my firſt paper as deficient in "philoſophical correctneſs," and wanting "the calm ſpirit of philoſophy." But in attempting to prove my errors, he has proved nothing but his own want of knowledge: and to thoſe who wiſh to ſee an example of C. L.'s "*calm ſpirit of philoſophy,*" I beg leave to recommend the peruſal of his letter in Vol. IX. p. 235, of this Journal.—Theſe ſtrange aſſertions are followed by another ſtill more ſtrange.

Quotation, in which C. L. denies Mr. W.'s facts.

He ſays, "*I will beg leave* to wave any attention to the hiſtory of Mr. Walker's experiments upon which I have animadverted, and *to deny his facts.*"

Thinking that I did not underſtand his meaning correctly, I examined the ſide-note, which runs thus: "C. L. declines any further diſcuſſion reſpecting Mr. Walker's experiments with lenſes, *but denies the facts.*"

Mr. W.'s reply.

As there is no reaſoning with a man who will deny facts, I ſhall decline examining the remainder of C. L.'s letter.

Your other anonymous correſpondent, who has denied the truth of my experiments, will be more eaſily anſwered.

Horizontal moon.

The opinions which have been advanced reſpecting the horizontal luminaries, ſince the days of Ptolomy, who was born about the year 70 of the Chriſtian æra, are ſo numerous, that

that it is ſcarcely poſſible to form a new one. This correſpondent has picked up the theory of Des Cartes, which was confuted more than 100 years ago by Mr. Molyneux. See Philoſ. Tranſ. abr. Vol. I. p. 221; or Dr. Hutton's Mathematical Dictionary, Vol. II. p. 74. I ſhall paſs over this writer's optical errors by juſt obſerving, that he has been doing nothing leſs than denying an eſtabliſhed property in optics.

Farther remarks on the image formed by a lens,

But before I proceed, it may be convenient to thoſe who are not in poſſeſſion of your two Numbers which contain my former papers on this ſubject, to know what that property in optics is, which has met with ſo much oppoſition.

aſſerted to vary with the aperture.

I have proved experimentally, that the image of a candle in the focus of a double convex lens, decreaſes in magnitude as the aperture of the lens is contracted *: And I have alſo advanced, that the image of the ſun or moon, in the focus of the object-glaſs of a teleſcope, decreaſes with its aperture †.

This poſition aſcribed to Newton,

Now it is well known to thoſe who underſtand optics, that this property, the truth of which has been denied ſo poſitively, was diſcovered by Sir Iſaac Newton: and a demonſtration of it may be ſeen in the Optics of Newton, Emerſon, Smith, and Martin, and alſo in the Encyclopædia Britannica, &c.

because he notes the ſpace of diffuſion in pencils of rays.

"The breadth of the leaſt circular ſpace into which object-glaſſes of teleſcopes can collect all ſorts of parallel rays, is about the $27\frac{1}{2}$ part of half the aperture of the glaſs, or 55th part of the whole aperture." (Newton's Optics, 2d Edit. p. 73). Hence it is evident, that if you contract the breadth of the object-glaſs from 10 to 1, the breadth of the circle of light, in its focus, will be contracted in the ſame proportion.

Whence it is deduced, that the image alſo will vary.

In my next paper, this intereſting property of light will be further explained.

I am, Dear Sir,

Your humble ſervant,

EZ. WALKER.

Lynn, March 16, 1805.

* Philoſophical Journal, Vol. IX. p. 164.

† Philoſophical Journal, Vol. X. p. 110.

XII.

Account of an Aeroſtatic Voyage, made by M. GAY-LUSSAC *on the 29th Fructidor, in the Year* 12. *Read to the National Inſtitute, on the 9th Vendemiaire, in the Year* 13.*

Motives of the aſcent.

IN the account of the firſt aeroſtatic voyage which M. Biot and I had the honour to ſubmit to the Inſtitute, we announced that, from the ſurface of the earth to the height of 3977 metres (13021.675 feet), the magnetic power did not experience any ſenſible diminution; and at the ſame time we ſignified a deſire to undertake new aſcents, to aſcertain this important fact at greater heights. We ſoon found that this was alſo the wiſh of many members of the Inſtitute; and, encouraged by the general intereſt which our firſt voyage had excited, we reſolved on making a ſecond very ſhortly; but our aeroſtat not being capable of carrying us together to a greater height than in our firſt aſcent, it was agreed between us that I ſhould go alone. From this moment all our attention was given to the inſtruments which it was requiſite for me to carry; and their conſtruction, which was again entruſted to M. Fortin, as well as an operation which the balloon underwent to give it more levity, retarded my departure till the 29th of laſt Fructidor.

Conſtruction of the inſtruments.

Inſtructed by the experience of our former aſcent, we made ſome changes in our inſtruments; and, in the firſt place,

Horizontal needle.

that the oſcillations of the horizontal needle might be leſs affected by the rotation of the balloon, we cauſed a new one to be conſtructed, only 15 centimetres in length. Thus, its oſcillations being more rapid than thoſe of the balloon, it would be eaſier to determine their duration.

Dipping needle.

We made greater modifications in the dipping needle. That we might not be obliged, at each obſervation, to replace it in the magnetic meridian, and to adjuſt its axis truly horizontal, we ſuſpended the metallic chape which ſupports it by a ſilken thread; and, to judge of its inclination, we fixed a portion of a tranſparent circle, graduated in diviſions, to the chape. The whole apparatus was very light, the tenſion on the ſilk thread but trifling, and the needle could reſume its

* From Annales de Chimie, No. 154, Fructidor, An. XIII.

meridian

meridian with facility. M. Coulomb, after having touched the needle, proved it by the method which he propoſed in the Memoirs of the Inſtitute; and he found that it gave an inclination of 70.5° of the common ſcale. In one of its poſitions, which was that in which it was to remain, it varied 31°.

Azimuth compaſs.

In our firſt aſcent, the under ſurface of the glaſs which covered our azimuth-compaſs was covered with water, and we were prevented from ſeeing the ſhadow of a horizontal thread, which ſerved us for a ſtyle. To avoid this inconvenience, it was ſufficient to remove the glaſs of the compaſs; in other reſpects, no alteration was made in its firſt diſpoſition. M. Lepine again ſupplied us with two ſecond-watches, one of which was a ſtop-watch: it was with the latter that I made all my obſervations.

Thermometer.

The thermometer I uſed, was the centigrade mercurial thermometer. To guard it from the action of the ſun, we placed it within two concentric cylinders of paſteboard covered with gilt paper, one of which was about four centimetres in diameter, and the other ſix.

Hygrometers.

Our hygrometers with four hairs, of the conſtruction of M. Richer, were diſpoſed in nearly a ſimilar manner. The two glaſs balloons in which I was to bring the air, were exhauſted to nearly a millimetre of mercury, and we were ſatisfied by leaving them in this ſtate for eight days, that they perfectly retained the vacuum. For fear of an accident, we had provided a third balloon of braſs; but very fortunately it was uſeleſs.

Barometers.

Our two barometers have not a conſtant level; and to get the true barometric heights, we formed a table of compariſon, by placing them under the receiver of the pneumatic machine, and meaſuring their depreſſion from five to five centimetres, by means of a ſtandard, the reſervoir of which has a conſtant level, and which is provided with a very good ſcale. Being no longer obliged to obſerve more than the upper level to get the true barometric heights, the number of obſervations was diminiſhed to one half; which is of great importance when the attention is divided between perſonal ſafety and delicate experiments.

Such nearly were the eſſential inſtruments I took with me in my voyage. I was alſo provided with an apparatus for determining the electricity of the air; but a few moments after I quitted the earth, I loſt the two metallic wires which were

The apparatus for meaſuring the electricity of the air rendered uſeleſs.

to

to attract the electricity at 50 and at 100 metres below me, and I could not make any use of it. It will be seen that we had avoided every thing which could influence our needles; and even our anchor, although suspended 50 metres below the boat, was of wood armed with copper.

This is not the place to mention the precautions which M. Conté had taken, that this new ascent might be free from danger; but it is to be wished that he would himself publish what a long and enlightened experience has taught him on this subject. For ourselves, we owe him a great tribute of gratitude for his trouble and the interest he has taken in our voyages; and we are bound to acknowledge, that if they have been so fortunate, we are indebted for it to his provident cares.

Departure.

All our instruments being ready, my departure was fixed for the 29th Fructidor: In fact, on that day I ascended from the Conservatory of the Arts and Manufactures, at 9^h 40′, the barometer being at 76.525 centimetres, the hygrometer at 57.5°, and the thermometer at 27.75°. M. Bouvard, who makes meteorological observations daily at the Observatory of Paris, judged the atmosphere to be greatly charged with vapour, but without clouds. In fact, I had scarcely risen 1000 metres when I perceived a light vapour diffused through all the atmosphere, which rendered the sight of remote objects confused.

State of the atmosphere on the earth.

First experiment on the oscillations of the horizontal needle.

Arrived at the height of 3032 metres (9929.8 F.), I began to make the horizontal needle oscillate, and this time I obtained 20 oscillations in 83″, while on the earth, and in other respects in the same circumstances, it required 84.33″ to make the same number *. Although my balloon was affected with the rotatory motion we had perceived in our first experiment, the rapidity of the motion of our needle permitted me to count 20, 30, and even 40 oscillations.

Inclination of the needle at a height of 12651.365 feet, the same as on the earth.

At the height of 3863 metres (12651.365 F.), I found the variation of my needle, taking the mean of the amplitude of its oscillations, was sensibly 31°, as on the earth. It re-

* Although I have here indicated hundredth parts of a second, I am aware that I cannot observe such small fractions; but I obtained them by divisions, because on the earth I generally made 30 oscillations, which required 126.5″.

quired much time and patience to make this obſervation; becauſe, although borne by the maſs of the atmoſphere, I perceived a ſlight wind which continually deranged the compaſs, and after ſeveral unſucceſsful attempts, I was obliged to give up making new ones. Nevertheleſs, I believe the obſervation I have ſtated above is entitled to ſome confidence.

The obſervations with the azimuth compaſs not to be depended upon.

Some time after I wiſhed to obſerve the dipping-needle; but behold what had happened. The drought, favoured by the action of the ſun in a rareſied air, was ſuch, that the compaſs was ſo much warped as to have bent the metallic circle on which the diviſions were traced, and to be itſelf crooked. The motions of the needle therefore had not the ſame freedom; but independently of this diſappointment, I had obſerved that it was very difficult to obſerve the dip of the needle with this apparatus. In ſhort, when I placed the compaſs ſo as to make the ſhadow of the horizontal thread, which ſerved as a ſtyle, coincide with a fixed line, the motion I had given to the compaſs communicated one to the needle, and by the time this was nearly reſtored to reſt, the ſhadow of the ſtyle no longer coincided with the fixed line. It was neceſſary to place the compaſs again in a horizontal poſition, and during the time which this operation required, every thing was again deranged. Unwilling to perſiſt in making obſervations on which I could place no reliance, I abandoned them entirely; and diveſted of every other care, I gave all my attention to the oſcillations of the horizontal needle. I am nevertheleſs convinced, by the knowledge of the defects of our compaſs, that it is poſſible to employ one more ſuitable, which would determine the dip with great preciſion. In trying this experiment, I lowered the other needles ſeparately in linen bags, to 15 metres below the boat.

Cauſes of its defects.

The experiment ſtill practicable.

The aggregate of the reſults formed into a table.

That the aggregate of all the reſults which I obtained may be more eaſily ſeen, I have collected them into a table which is at the end of this memoir; and they are placed there as they occurred to me, with the correſponding indications of the barometer, thermometer, and hygrometer. The heights have been calculated, according to the formula of M. Laplace, by M. Gouilly, engineer of bridges and highways, who has been ſo kind as to take this trouble. The barometer not having varied ſenſibly on the day of my aſcent, from ten o'clock to three, to calculate the different heights at which I made

Calculation of the heights by the barometer compared with one on the earth:

made my obſervations, the height of the barometer on the earth has been taken at 70.568 centimetres, which was the caſe at three o'clock; a height which, conformably to the obſervations made by M. Bouvard at the Obſervatory, is greater by 0.43 millimetres than that which had been obſerved at the moment of my departure. The heights of the barometer in the atmoſphere have been brought to thoſe which would have been indicated by a barometer with an uniform level placed in the ſame circumſtances, and the mean between the obſervations of the two barometers has been taken for each height. The temperature on the earth having alſo varied very little between ten and three o'clock, it has been ſuppoſed conſtant and equal to 30.75° of the centigrade thermometer.

and of the temperature.

The irregular variation of the temperature ariſes from the change of place during the obſervation.

Now on looking into the table it will be evident, in the firſt place, that the temperature follows an irregular courſe with reſpect to correſponding heights; which, no doubt, is occaſioned by the obſervations having been made ſometimes in aſcending, and ſometimes in deſcending, and the thermometer having followed theſe variations too ſlowly. But if the degrees of the thermometer alone are conſidered, which form with each other a continually decreaſing ſeries, a more regular law will be found. Thus the temperature on the earth being 27.75°, and 8.5° at the height of 3691 metres, if the difference of the heights is divided by that of the temperatures, we ſhall at once obtain 191.7 metres (627.8175 feet) of elevation for each degree of the reduction of the temperature. By performing the ſame operation for the temperatures 5.25° and 0.5°, as well as for thoſe of 0° and −9.5°, we ſhall find, in both caſes, 141.6 metres (463.74 feet) of elevation for each degree of the reduction of the temperature: which ſeems to indicate that near the ſurface of the earth the heat is governed by a law which decreaſes leſs than at an elevation in the atmoſphere, and that at length it follows a decreaſing arithmetical progreſſion. If it be ſuppoſed that from the ſurface of the earth, where the thermometer was at 30.75° to the height of 6977 metres (22849.675 feet), where it had fallen to −9.5°, the heat had diminiſhed as the elevations increaſed, an elevation of 173.3 metres (567.557 feet) will correſpond to each degree of the reduction of the temperature.

Difference of elevation correſponding to a degree of the difference of temperature.

The

The courſe of the hygrometer was very ſingular. At the ſurface of the earth it was only at 57.5°, while at the height of 3032 metres it marked 62° : from this point it was continually falling to the height of 5267 metres, where it indicated no more than 27.5° ; and from thence to the height of 6884 metres, it roſe gradually to 34.5°. If, by theſe reſults, it is wiſhed to determine the law of the quantity of water diſſolved in the air at different elevations, it is clear that attention muſt be paid to the temperature ; and on joining this conſideration with it, it will be ſeen that it follows an extremely decreaſing progreſſion.

Unſteady courſe of the hygrometer.

If now the magnetic oſcillations are conſidered, it will be obſerved that the time for ten oſcillations, made at different heights, is ſometimes above and ſometimes below 42.16″, which they require upon the earth. By taking the mean of all the oſcillations made in the atmoſphere, ten oſcillations would require 42.20″, a quantity which differs very little from the preceding; but on conſidering only the laſt obſervations which were made at the greateſt heights, the time for ten oſcillations would be a little leſs than 42.16°, which would indicate, on the contrary, that the magnetic power had been a little augmented. Without meaning to draw any conſequence from this apparent ſlight increaſe, which may very probably ariſe from the errors to which experiments of this deſcription are liable, I conclude that the total of the reſults I now offer, confirms and extends the fact which M. Biot and myſelf had obſerved, and which, as well as the univerſal gravitation, proves that the magnetic power does not experience ſenſible variations at the greateſt heights to which we can aſcend.

Magnetic oſcillations at different heights.

The magnetic power is not ſenſibly altered at the greateſt elevations.

The conſequence we have drawn from our experiments, will appear a little too precipitate to thoſe who recollect that we were unable to make experiments on the inclination of the touched needle. But if it be obſerved that the power which cauſes an horizontal needle to oſcillate, is neceſſarily dependent upon the intenſity and direction of the magnetic power itſelf, and that it is repreſented by the co-ſine of the angle of inclination of this latter power, they cannot avoid concluding with us, that, ſince the horizontal power has not varied, neither can the magnetic power have varied, unleſs they ſuppoſe that the magnetic power can have varied preciſely

Probable objections to this concluſion refuted.

ciſely in oppoſition to, and in the ſame proportion as the coſine of its inclination, which is by no means probable. In ſupport of our concluſion we have, beſides, the experiment on the inclination made at the height of 3863 metres (12651.325 feet), which proves that, at that elevation, the variation did not vary in a ſenſible manner.

Further proofs of the magnetiſm being the ſame as on the earth.

When at the height of 4511 metres, I preſented to a ſmall touched needle, and in the direction of the magnetic power, the lower extremity of a key; the needle was attracted, and afterwards repelled by the other extremity of the key, which I brought down parallel to itſelf. The ſame experiment, repeated at 6107 metres, had the ſame iſſue: another very evident proof of the action of terreſtrial magnetiſm.

Heights at which the air was taken for examination.

At the height of 6561 metres (21487.275 feet), I opened one of my two balloons of glaſs; and at 6636 metres (21733.9 feet), I opened the ſecond: the air entered both with a hiſſing noiſe. Finally, at 3^h 11′, the aeroſtat being perfectly full, and having only 15 kilogrammes of ballaſt, I

Greateſt elevation.

determined to deſcend. The thermometer was then at 9.5° below the temperature of melting ice, and the barometer at 32.88 centimetres; which gives for my greateſt elevation above Paris, 6977.37 metres (22850.887 feet), or 7016 metres (22977.4 feet) above the level of the ſea.

Effects on the animal economy at this height.

Although well clothed I began to feel the cold, particularly in my hands, which I was obliged to keep expoſed to the air. My reſpiration was ſenſibly obſtructed; but I was far from feeling any inconvenience ſufficient to induce me to deſcend. My pulſe and my reſpiration were greatly accelerated; hence from reſpiring very frequently in a very dry air, I could not be ſurpriſed at my throat being ſo dry, that it was with difficulty I ſwallowed bread. Before ſetting out I had a ſlight head-ach, ariſing from the fatigues of the preceding day and the want of reſt in the night, and I retained it all the day, without perceiving that it was increaſed. Theſe are all the inconveniences I experienced.

Clouds at a very great height during this aſcent.

A phenomenon which ſtruck me at this great height, was the ſeeing clouds above me, and at a diſtance which appeared to me to be very conſiderable. During our firſt aſcent the

Their extreme height in the former one.

clouds did not riſe above 1169 metres (3828.475 feet), and above, the ſky was in its greateſt purity. Its colour at the zenith was ſo intenſe, that it might be compared with that of

Pruſſian

Pruſſian blue; but in the laſt voyage which I made I ſaw no clouds below me; the ſky was very vaporous, and its colour generally dull. It is perhaps not uſeleſs to obſerve, that on the day of our firſt aſcent the wind was north-north-weſt, and on the laſt, ſouth-eaſt. Direction of the wind.

As ſoon as I perceived that I began to deſcend, I thought only of moderating the deſcent of the balloon, and of rendering it extremely ſlow. At 3^h 45′ my anchor touched the ground and held, which gives 34′ for the time of my deſcent. The inhabitants of a ſmall hamlet in the vicinity ſoon ran to the ſpot, and while ſome brought the balloon towards them by drawing the rope of the anchor, others, placed under the boat, waited impatiently till they could reach it with their hands, to catch and depoſit it upon the earth. My deſcent was therefore effected without the ſlighteſt ſhock or the leaſt accident, and I do not believe that a more fortunate one is poſſible. The ſmall hamlet beſide which I deſcended, is called Saint-Gourgon; it is ſituated ſix leagues north-weſt from Rouen. Deſcent.

On my arrival at Paris, my firſt care was to analyze the air I had brought. All the experiments were made at the Polytechnic School, under the eyes of MM. Thenard and Greſſet, and I truſted as much to their judgment as to my own. We noticed the diviſions of the eudiometer alternately, and without communication, and it was only when we were perfectly agreed that we wrote them down. The balloon which was filled with air at 6636.5 metres (21735.537 feet), was opened under water, and we all judged that it had at leaſt filled the half of its capacity; which proves that the balloon had retained the vacuum very well, and that no foreign air had entered it. We purpoſed to have weighed the quantity of water entered into the balloon, to compare it with its capacity; but not having the neceſſary inſtruments at hand, and our impatience to know the nature of the air it contained being very great, we did not make that experiment. At firſt we uſed Volta's eudiometer, and we analyzed it comparatively with atmoſpheric air taken in the middle of the court-yard of the Polytechnic School. The following is the comparative analyſis of theſe two airs. Analyſis of the air taken at great elevations.

Analyſis

	Analyſis of the atmoſpheric Air.		*Analyſis of the Air taken at an Elevation of 6636 Metres.*	
		Meaſures.		Meaſures.
First Experiment.	Atm. Air,	3.	Air, - -	3.
	Hyd. Gas,	2.	Hyd. Gas, -	2.
	Reſidue after the combuſtion, -	3.04	Reſidue, -	3.05
		Meaſures.		Meaſures.
2d Experiment.	Atm. Air,	3.	Air, - -	3.
	Hyd. Gas,	2.	Hyd. Gas, -	2.
	Reſidue,	3.05	Reſidue, -	3.04

Atmoſpheric air and air taken at a great elevation, are identically the ſame.

At the ſame time one meaſure of very pure oxigen gas required 2.04 meaſures of hidrogen gas; and this reſult differing only .01 from that obtained in experiments made on a very large ſcale, and with great care, in the compoſition of water, it is evident that great confidence may be placed in our reſults. They prove therefore that atmoſpheric air, and air taken at an elevation of 6636.5 metres, are identically the ſame, and that each of them contains .2149 of oxigen. On analyzing the latter air by hydro-ſulphuret of potaſh, we found in it .2163 of oxigen. I cannot give the reſult of the comparative experiment made on atmoſpheric air, becauſe we were unable to collect it; but the proportion of oxigen which I have indicated, is alſo a little greater than that given by the combuſtion of hidrogen gas, and is comprized within the limits of the variations found in the compoſition of the atmoſphere at the ſurface of the earth, and which do not prevent it from being conſidered as conſtant.

This fact proved by other experiments.

The identity of the analyſes of the two airs made by hidrogen gas, proves directly, that that which I brought did not contain the latter gas; nevertheleſs I alſo ſatisfied myſelf of it, by burning with the two airs only a quantity of hidrogen gas leſs than would have been requiſite to abſorb all the oxigen gas; for I found that the reſidues of the combuſtion of the two airs with the hidrogen gas were exactly the ſame.

Sauſſure

Sauſſure junior alſo found, by uſing nitrous gas, that air taken in the Col-du-Geant contained, within one hundredth part, as much oxigen as that of the plain; and his father aſcertained the preſence of carbonic acid on the ſummit of Mont-Blanc: Moreover, the experiments of MM. Cavendiſh, Macarty, Berthollet, and Davy, have confirmed the identity of the compoſition of the atmoſphere over all the ſurface of the earth. It may therefore be concluded generally, that the conſtitution of the atmoſphere is the ſame from the ſurface of the earth to the greateſt heights to which we can aſcend. The compoſition of the atmoſphere is every where the ſame.

Theſe are the two principal reſults which I obtained from my laſt voyage. I have aſcertained the fact which had been obſerved by M. Biot and me, on the ſenſible permanence of the intenſity of the magnetic power at a diſtance from the ſurface of the earth; and beſides, I think I have proved that the proportions of oxigen and azote which conſtitute the atmoſphere, do not vary ſenſibly in the moſt extended limits. Many things ſtill remain to be elucidated in the atmoſphere, and we hope the facts we have already collected will ſo far intereſt the Inſtitute as to induce them to deſire us to continue our experiments. Recapitulation of the reſults.

TABLE

TABLE OF OBSERVATIONS.

Temperature expreſſed in degrees of the centigrade thermometer.	Mean of the indications of tne two hygrometers.	Mean height of the barometer in the atmoſphere brought to that of a barometer with a conſtant level.	Correſponding heights in metres above Paris.	Correſponding heights in Engliſh Feet.	Number of magnetic oſcillations.	Duration of oſcillations expreſſed in ſeconds.	Oſcillations reduced to the common number 10.	Correſponding times.	OBSERVATIONS.
°	°	Cent.				//		//	
27.75	57.5	76.525	0.0	0.0	30	126.5	10	42.16	The firſt line gives the obſervations which were made on the earth before the departure.
12.50	62.0	53.81	3032.01	9929.803	20	83.0	10	41.5	
11.00	50.0	51.43	3412.11	11274.660					
8.50	37.3	49.68	3691.52	12089.073					
10.50	33.0	49.03	3816.79	12499.987	10	42.0	10	42.0	
		45.28	4511.61	14775.522	30	127.5	10	42.5	
12.0	30.9	46.66	4264.65	13966.728	30	125.5	10	41.8	
10.0	29.9	46.26	4327.86	14173.741	20	86.0	10	43.0	
8.25	27.6	44.04	4725.90	15469.322	20	84.5	10	42.2	
6.50	27.5	43.53	4808.74	15746.200	30	128.5	10	42.8	All the heights muſt be augmented by at leaſt 39 metres (20 toiſes), if it is wiſhed to reckon them from the level of the ſea.
8.75	29.4	45.28	4511.61	14775.522	30	127.5	10	42.5	
5.25	30.1	42.49	5001.85	16381.058					
4.25	27.5	41.14	5267.73	17251.815	40	169.0	10	42.2	
2.5	32.7	39.85	5519.16	18069.249					
0.5	30.2	39.01	5674.85	18585.133					
1.0	33.0	41 41	5175.06	13498.321	30	126.5	10	42.1	
— 3.0	32.4	37.17	6040.70	19783.292					
— 1.5	32.1	36.96	6107.19	20001.047	20	84.0	10	42.0	
0.0	35.1	39.18	5631.65	18387.343	30	127.5	10	42.5	
— 3.25	33.9	36.70	6143.31	20119.340	20	82.0	10	41.0	
— 7.0	34.5	33.39	6884.14	22545.558	20	83.5	10	41.7	
— 9.5		32.88	6977.37	22850.887					

Expériences

XIII.

Experiments and Obſervations on the Motion of the Sap in Trees. In a Letter from THOMAS ANDREW KNIGHT, *Eſq. to the Right Hon.* SIR JOSEPH BANKS, *Bart. K. B. P. R. S. From the Philoſophical Tranſactions for* 1804.

MY DEAR SIR,

IN the Obſervations on the Deſcent of the Sap in Trees, which I laſt year took the liberty to requeſt you to lay before the Royal Society, I offered a conjecture, that the veſſels of the bark, which paſs from the leaves to the extremities of the roots, were, in their organization, better calculated to carry the fluids they contain towards the roots than in the oppoſite direction. I had not, however, at that time, any experiment directly to ſupport this ſuppoſition; but I thought the forms generally aſſumed by trees in their growth, evinced the compound and contending actions of gravitation, and of an intrinſic power in the veſſels of the bark, to give motion to the fluid paſſing through them. In the account of the experiments which I have now the honour to addreſs to you, I truſt I ſhall be able to adduce ſome intereſting facts, in ſupport of that inference.

Experiments and obſervations on the motion of the ſap in trees.

Having ſelected, in the ſpring of 1802, four ſtrong ſhoots of the vine, growing along the horizontal trellis of my vinery, I depreſſed a part of each ſhoot, whilſt it was ſoft and ſucculent, about three inches deep, into the mould of a pot placed beneath it for that purpoſe; but without making any wound, or inciſion, in the young ſhoots thus employed as layers.

In this poſition they remained during the ſucceeding ſummer; and, in the autumn, had nearly filled the pots, which were ten inches in diameter, with their roots. As ſoon as the leaves had fallen, the layers were diſengaged from the parent ſtocks; and about five inches of wood, containing one bud, were left both at the proper and the inverted end of each layer. Every bud was alſo, by previous management, made to ſtand at an equal diſtance from the mould in the pots, and with an equal elevation, of about thirty-ſix degrees. About one inch of wood was likewiſe left at each end of every layer beyond the buds.

Experiments and obſervations on the motion of the ſap in trees.

In the ſucceeding ſpring, the buds vegetated ſtrongly, both at the proper and at the inverted ends of the layers, as the experiments of Hales and du Hamel had given me reaſon to expect; and, in one inſtance, the bud at the inverted end of the layer grew with greater vigour than that at its proper end: but the growth of theſe buds was not the object which I had in view.

I have already ſtated, that nearly an inch of wood was left at each end of every layer, beyond the bud; and, to this wood, at the inverted ends of the layers, my attention was chiefly directed: for, if the veſſels of the bark poſſeſſed the powers I attributed to them, I concluded that the ſap would be impelled to the inverted ends of the layers, and be there employed in the production of new wood, and roots; and, in this, my expectations were not diſappointed. At the proper end of the layers, the wood immediately beyond the buds became dry and lifeleſs, early in the ſucceeding ſummer; the ſtems alſo, between the buds and the mould in the pots, increaſed in ſize as uſual; and nothing peculiar occurred. But, at the inverted end, appearances were extremely different: new wood here accumulated rapidly beyond the buds, and numerous roots, of conſiderable length, were emitted, whilſt no ſenſible growth took place between the baſe of the young ſhoots and the mould in the pots.

It having been proved by Du Hamel, that inverted parts of trees readily emit roots, I expected to derive further information from cuttings of this kind: I therefore planted, in the autumn of 1802, forty cuttings of the gooſeberry-tree, and an equal number of the common currant-tree; one half of each being inverted. Of the former, not one of the inverted cuttings ſucceeded; whereas few of the latter failed; and in theſe I had an opportunity of obſerving the ſame accumulation of wood above the baſes of the annual ſhoots, and the ſame mode of growth, in every reſpect, as in the inverted vines; except that no roots were emitted at their upper ends. The ſame thing occurred, without any variation, in inverted grafts of the apple-tree.

If it be admitted, according to the theory I have on a former occaſion laid before you, that the ſap deſcends from the leaves through the veſſels of the bark, and that ſuch veſſels are, in their organization, better calculated to carry their contents towards the original roots than in the oppoſite direction, it will be extremely eaſy to explain the cauſe of the accumulation of wood,

wood, and the emiſſion of roots, above, inſtead of below, the baſe of the annual ſhoots. The veſſels of the bark (the *vaiſſeaux propres* of Du Hamel) commencing in the leaves, were formerly traced by M. Mariotte, and ſubſequently by myſelf, (being ignorant of his diſcovery,) to the extremities of the roots; and, when a cutting or tree is planted in its natural poſition, the ſap paſſes downwards through theſe, to afford matter for new roots, and to increaſe the bulk of thoſe already formed, having given proper nutriment to the branches and trunk in its deſcent. But, in the inverted cutting, or tree, theſe veſſels become inverted; and, if their organization be ſuch as I have ſuppoſed it, a conſiderable part of that fluid, which naturally deſcends, will be carried upwards, and occaſion the production of new wood, above, inſtead of below, the junction of the annual ſhoot with the older wood, as in the experiments I have deſcribed. The force of gravitation will, however, ſtill be felt; and by its agency, ſufficient matter to form new roots may be conveyed to thoſe parts of the inverted cutting, or tree, which are beneath the ſoil. Beſides, if we ſuppoſe a variation to exiſt in the powers or organization of the veſſels which carry the ſap towards the root, we may alſo attribute, in a great meaſure, to this cauſe, the different forms which different ſpecies or varieties of trees aſſume; for, if the fluid in theſe veſſels be impelled with much force towards the roots, little matter will probably be depoſited in the branches, which, in conſequence, will be ſlender and feeble, as in the vine; and there is not any tree that has been the ſubject of my experiments, in which new wood accumulated ſo rapidly at the upper end of inverted plants. To an exceſs of this power, in the veſſels of the bark, we may alſo aſcribe the peculiar growth of what are called weeping trees; for, by this power, the effects of gravitation will be, in a great degree, ſuſpended; and the pendant branch will continue healthy and vigorous, by retaining its due circulation. The perpendicular branch will, however, ſtill poſſeſs ſome advantages; for in this, gravitation will act on the fluid deſcending from the leaves; and theſe will of courſe abſorb from the atmoſphere with increaſed activity. A greater quantity of matter will therefore enter, within any given portion of time, into veſſels of the ſame capacity; and this increaſed quantity may frequently exceed that which the veſſels of the bark are immediately prepared to carry away. Much new wood will in conſequence be generated,

Experiments and obſervations on the motion of the ſap in trees.

Experiments and obſervations on the motion of the ſap in trees.

and increaſed vigour given; and, the ſame cauſes operating through ſucceſſive ſeaſons, will give the aſcendancy we generally obſerve in the perpendicular branch.

In the preceding experiments, none of the layers, or cuttings, exceeded a few inches in length; and, to the ſummit of theſe the ſap appeared to riſe, through the inverted tubes of the wood, nearly as well as in thoſe which retained their natural poſition. But ſome former experiments had induced me to ſuſpect, that this would not be the caſe in longer cuttings; I therefore planted in the autumn of 1802, twelve cuttings of the ſwallow, (*Salix caprea,*) inverting one half of them. The whole readily emitted roots, and grew with luxuriance; but their modes of growth were extremely different. In the cuttings which ſtood in their natural poſition, vegetation proceeded with moſt vigour at the points moſt elevated; but in the inverted cuttings, it grew more and more languid as it became diſtant from the ground, and nearly ceaſed, towards the concluſion of the ſummer, at the height of four feet. The new wood alſo, which was generated by theſe inverted cuttings, accumulated above the baſes of the annual ſhoots, as in the preceding inſtances.

Theſe facts appear to prove, that the veſſels of plants are not equally well calculated to carry their contents in oppoſite directions; and, I think, afford ſome grounds to ſuſpect that the veſſels of the bark, like thoſe which conſtitute the venous ſyſtem of animals, (to which they are in many reſpects analogous,) may be provided with valves, whoſe extreme minuteneſs has concealed them from obſervation.

The experiments, and ſtill more the plates, of Hales, have induced naturaliſts to draw concluſions in direct oppoſition to the preceding. But the Plates of that great naturaliſt are not always taken correctly from nature; * and Plates, under ſuch circumſtances, however fair and candid the intentions of an author may be, will too often be found ſomewhat better calculated to ſupport his own hypotheſis, than to elucidate the facts he intends to ſtate.

The preceding peculiarities in the growth of inverted cuttings, appear to have eſcaped the obſervation of Du Hamel; and, as very few inſtances of error, or want of accurate obſervation,

* The eleventh Plate (Vegetable Staticks) is that to which, in this place, I particularly allude.

will

will ever be found in the works of that excellent naturaliſt, I muſt requeſt permiſſion to ſend you ſome of the ſubjects of my experiments, as vouchers for my own accuracy.

Experiments and obſervations on the motion of the ſap in trees.

Of the inverted cuttings employed by Du Hamel, a ſmall portion only appears to have remained above the ground; and under ſuch circumſtances, the different forms of thoſe growing in their natural, or inverted, poſition would be ſcarcely obſervable. It appears alſo, from his experiments, that ſuch inverted cuttings, in ſubſequent years, grow with as much vigour as others that are not inverted; whence we muſt conclude, that the organization of the internal bark becomes again inverted, and adapted to the poſition of the branch. The growth of ſome inverted plants of the gooſeberry-tree, which I obtained, many years ago, from layers, gave me reaſon to draw a different concluſion; for theſe always continued weak and dwarfiſh. I do not, however, entertain the ſlighteſt degree of doubt, but that the aſſertion of Du Hamel is perfectly correct.

I intended to have added ſome obſervations on the reproduction of buds and roots of trees; but theſe would neceſſarily extend the preſent Paper to an immoderate length; I ſhall therefore reſerve them for a future communication, and conclude with an account of an experiment which more properly belongs to the Paper I had the honour to addreſs to you laſt year, but which had not then ſucceeded.

I have ſtated, in that Paper, that the leaf-ſtalk, the fruit-ſtalk, and the tendril, of the vine, had been ſucceſsfully ſubſtituted, in many inſtances, for each other; but that I had failed in my efforts to engraft a bunch of grapes, by approach, on the leaf-ſtalk; owing, I conceived, to the operation having been improperly performed. In thoſe experiments, I cut the leaf-ſtalk into the form of a wedge, and made an inciſion in the fruit-ſtalk, adapted to receive it; but, under ſuch circumſtances, the leaf-ſtalk (as I had proved by many experiments) has no power to generate new matter; and the wounds of the fruit-ſtalk heal ſo ſlowly, that I readily anticipated the ill-ſucceſs of the operation. In the laſt ſpring, I pared off ſimilar portions of the leaf-ſtalk and fruit-ſtalk; and, bringing the wounded parts into contact, I ſecured them cloſely together, by means of a bandage, letting the leaf remain. Under theſe circumſtances, an union took place; and the fruit-ſtalk being then taken off below the point of junction, and the leaf-ſtalk above it, the grapes drew

their

Experiments and obſervations on the motion of the ſap in trees.

their whole nutriment through the remaining part of the leaf-ſtalk. They did not, however, acquire their full ſize; and the ſeeds were ſmall, and, I think, incapable of vegetating; but this I attribute to the want of nutriment in quantity rather than in quality; for the union of the veſſels of the leaf-ſtalk with thoſe of the fruit-ſtalk was very imperfect. The grapes, which were the purple Frontigniac, poſſeſſed their muſky flavour, in the ſame degree with others growing on the ſame plant.

There is another experiment in my laſt Paper, which I will alſo notice here, becauſe it appears to lead to ſome important concluſions, and had been tried only in a ſingle inſtance. I have there ſtated, that the ſtem of a young tree became elliptical, by being confined to move only in the ſegment of a large circle. This experiment was ſucceſsfully repeated during the laſt year, on other trees; but I have nothing to add to the deſcription which I have already given.

I am, &c.

T. A. KNIGHT.

XIV.

Extract of a Memoir of Mr. ERMAN, *entitled, Obſervations and Doubts concerning Atmoſpheric Electricity* *.

MR. ERMAN who is already ſo advantageouſly known to philoſophers, has publiſhed towards the end of laſt year, a memoir which appears to deſerve their attention in an uncommon degree.

Signs of atmoſpheric electricity differ according to the inſtrument.

When he made experiments upon the electricity of the atmoſphere, he obſerved a great difference between the reſult preſented by an electrometer armed with a conductor which was ſuddenly raiſed from the earth, according to the known method, and that of a metallic rod of much greater length which was inſulated and fixed.

Deſcription of the electrometer.

The electrometer he uſed was that diſtinguiſhed in Germany by the name of the electrometer of Weiſs. The length of its leaves of gold is half an inch, and the diameter of the glaſs tube which encloſes them is three quarters of an

* Journal de Phyſique, Thermidor 12, or Vol. LIX. 98.

inch

inch in diameter, the height being an inch and a half. Its cover of ivory does not project above the glaſs, and is perforated in the middle with a hole in which a ſmaller glaſs tube is fixed, and through this laſt tube paſſes the metallic rod that ſerves to ſuſpend the gold leaves, and is upon the whole five feet in length, being compoſed with ſeveral pieces ſcrewed together, in order that they may be more eaſily conveyed from place to place by ſeparating them. A divergence of three lines in theſe leaves anſwers to two lines in the electrometer of Volta; and that degree of electricity which would produce a divergence of two lines and a half in this laſt, cauſes the leaves of Mr. Weiſs to touch the ſides of the glaſs cylinder.

When raiſed ſuddenly it diverges *plus*.

Mr. Erman walked in the fields with his electrometer, having its rod confined to the length of three feet. When he ſuddenly raiſed it from the ground, he obſerved a ſtrong divergence of *plus* electricity. When he lowered it with the ſame ſwiftneſs, he perceived an equal great divergence of *minus*. The point of a fixed metallic rod of much greater length, erected at the ſame ſpot, did not give the leaſt indication of electricity.

If ſlowly raiſed it does not diverge; but if then ſuddenly depreſſed it diverges *minus*.

An electrometer placed on a poſt of three feet in length, produced no divergence; and, when it was afterwards ſlowly raiſed one foot, or a foot and a half, it likewiſe afforded no ſign; but when it was very quickly brought down to the poſt, it exhibited *minus* electricity to ſuch a degree, that the gold leaf touched the ſides of the glaſs. This charge was gradually diſperſed in the air, or by touching the electrometer with the hand *; but by raiſing the inſtrument with equal ſpeed above the poſt, a divergence of *plus* electricity of the ſame ſtrength was afforded which was deſtroyed in the ſame manner.

Conducting power of the air interrupts the reſults.

The more inſulating the air the leſs it is neceſſary to raiſe the electrometer in order to produce this effect; but the longer the point, the more ſtrikingly will the effect be ſhewn, even in damp weather. A circular motion at a conſtant diſtance above the earth does not exhibit any electricity, neither does a progreſſive motion upon level ground afford any change;

* From this fact it appears, that the electrometer had not any ſlips of metal within the tube like thoſe of Bennet, who firſt uſed gold leaves. N.

but

but when the ground riſes even inſenſibly, the phenomenon is obſerved, and it is rather extraordinary, that it even affords a means of aſcertaining the level.

Variations of the experiment.

To vary the experiment, the electrometer may be placed upon a low ſupport, and touched with an inſulated ſtick or wire which has been quickly moved towards the ground, and is afterwards raiſed with the ſame quickneſs. The electrometer in the firſt caſe exhibits *plus* electricity, and in the ſecond *minus*.

It is very remarkable, that the *minus* electricity paſſes through Zero before it changes into *plus*, and *vice verſa*, accordingly as the inſtrument is raiſed or lowered even when there is no interval between the two motions; but the divergence will be more viſible, if time be allowed for the electrometer to diſcharge itſelf between the two experiments.

Sauſſure's obſervations.

Sauſſure formerly obſerved the eſſential requiſites in this phenomenon; but his electrometer was not ſo ſenſible, and he explained the phenomenon by ſuppoſing that the electrometer was the moſt inſulated, and the fixed rod not ſufficiently ſo to indicate ſuch ſlight degrees of electricity.

The electrometer is affected not by any electricity in the atmoſphere, but ſimply by the vicinity of the earth which appears to poſſeſs the *minus* ſtate.

Mr. Erman has aſcertained, that the phenomenon muſt be attributed to the manner in which the electricity is diſtributed in bodies. The maſs of the ground exerts its influence very ſenſibly to a certain diſtance, and the electric atmoſpheres act upon each other without any intervention of the air. The inſulated metallic rod which we raiſe from the ground by holding it perpendicularly, undergoes no other change than is owing to its own proper electricity, which is then leſs compenſated by the influence of the ground. Whether the electrometer or the plate of an electrophorus be ſlowly raiſed, the electricity becomes communicated to the ſurrounding air, and is not manifeſted. A ball placed on the point of the rod of the electrometer does not at all change the divergence of the leaves; and even when the conducting rod of the electrometer is encloſed in a tube of glaſs not open at the top, the phenomena are the ſame, and the divergencies of *plus* and *minus* are equally ſeen according to the direction of the motion. And when a ſecond tube is put over the firſt, for the purpoſe of more exactly preventing the contact of the air, the reſults are not at all changed.

The termination of the conductor or even its being encloſed in glaſs, makes no difference.

Mr. Erman procured a tube of glaſs 14 feet long, which he covered with varniſh, and through which he introduced a metallic wire which paſſed above the upper part, and had the electrometer adapted to its lower extremity. A ſmall piece of amber being rubbed and applied to the upper end of the wire, produced a divergence in the leaves. The electricity of the air had not, however, any influence on this fixed, and very nicely inſulated point; which ſhews, that the air ſurrounding a point is more adapted to deprive it of electricity than to communicate it. Another electrometer which was raiſed from the earth at the ſame time produced a great divergence.

A fixed rod not affected, becauſe its ſituation with regard to the earth is not changed.

Mr. Erman wiſhed to know whether two bodies in the ſame atmoſpheric ſtratum reciprocally modify their electric ſtate, when their diſtance is altered in either direction. Two gold leaf electrometers equally ſenſible, and armed with rods three feet long, one of which was bended, ſo that its point could be placed near that of the other electrometer, on the ſame horizontal line were held at arms length from each other, and touched with the finger to diſcharge, and put them in equilibrium with the ſurrounding air. They were afterwards brought towards each other horizontally, and when they were ſufficiently near to permit their atmoſpheres to act on each other, there was a divergence of *minus* electricity on both ſides, which came to its maximum when the electrometers were in contact. They were again ſeparated, and the divergence diſappeared, both being at the ſtate of Zero. One of the wires of theſe electrometers was put in contact with the earth, when the divergence of the other inſulated electrometer ſuddenly diminiſhed to half its former quantity; a fact that alſo proves that the whole depends upon the electricity inherent in bodies, and not at all upon that of the atmoſphere.

Two electrometers brought into contact, and then horizontally ſeparated affect each other according to the laws of bodies actually electrified.

When the electrometers were removed from each other, that which was touched with the finger indicated *plus* in a degree equal to the *minus* it had before ſhewn. If the two electrometers be touched with the finger, no further appearance of the *minus* divergence is ſeen in either of them; but if they be then ſeparated both will exhibit *plus*. Can theſe phenomena be attributed to the air? or is it poſſible to avoid admitting in theſe experiments the influence of electric atmoſpheres, and the law of the condenſer?—It is, therefore, well

Continuation of the experiment.

well proved, that all bodies, even which are in equilibrium with the earth, have electric atmoſpheres in the open air, from which reſults a modification in their electric ſtate.

The electric influence of a tree was ſtill greater.

Mr. Erman conjectured, that a body of a greater volume might have ſtill more influence. He obſerved a tree which ſtood alone in an open ſpace. He retired to the diſtance of twenty paces from it, and diſcharged the electricity of his electrometer by touching it with the finger. He then approached the tree, keeping his electrometer at the ſame height above the earth. The electrometer diverged negatively, and the divergence continually augmented as he came nearer, ſo that the leaves of gold touched the ſides of the glaſs at the moment when he was under the tree. This negative ſtate laſted as long as he remained there, but when he retired again it gradually diſappeared, ſo as to have become inſenſible when he arrived at the original diſtance of twenty feet.

When the communication between the point of the electrometer and the earth was completed under the tree, the divergence ceaſed; but the electricity was only apparently deſtroyed, and manifeſted itſelf again when he retired from the tree.

The mutual influence of two electrometers is ſcarcely ſeen very near the earth. Remarks on aeroſtatic experiments propoſed.

Two electrometers being brought near each other at the diſtance of half a foot from the earth, did not produce this effect, becauſe the maſs of the earth too greatly influenced that proximity. In order to have very ſimple reſults, it would be requiſite to diſcover ſome means of rapidly elevating an inſulated body to ſome thouſands of feet above the ground; and if this experiment were undertaken in the boat of a balloon, it would probably be ſeen, that the poſitive charge would conſtantly increaſe during the aſcent, and would become negative as it approached the earth: but if means could be had of changing the ſyſtem of the balloon in the atmoſphere, by the intervention of another body, which would be difficult, the contrary effects might be expected. This change of ſyſtem is however ſeen when a cloud deſcends, and diſcharges itſelf with exploſion into the earth. But an electrometer of the uſual conſtruction in the boat of a balloon would indicate nothing, becauſe the upper part always has a charge of electricity of the ſame kind as that of the gold-leaves, and of equal intenſity.

If the influence of the electricity of the ground be sufficient to prevent the divergence, the same effect ought to take place in every close chamber, because a roof or ceiling may be considered as a prolongation of the ground. Hence it is that this polarity does not manifest itself in a chamber; it is also erroneous to pretend that the atmosphere produces electricity; and it is equally erroneous to assert, that electric repulsion does not take place in a vacuum; Mr. Erman proposes to establish in an incontestible manner in another memoir, that the repulsive force of electricity, as well as of magnetism, are also manifested in a vacuum.

These effects cannot take place in a chamber, &c.

If the electricity of the rod of the electrometer could be ascribed to that of the atmosphere, it would follow, that it should be equally electric through its whole length; but if it depend on the earth, the rod will not be equally electrified, and its different parts will be variously modified accordingly as they may be more or less remote from the earth; and this in fact is the case. Two electrometers, each having a pointed rod of three feet, one of which was twice bended in a right angle, so as to have an horizontal portion of three or four inches before it proceeded upwards, were held in such a manner, that the point of the straight rod of the one touched the horizontal part or elbow of the other, which was lower. When they were touched, to discharge them, no divergence was seen, but when they were afterwards separated horizontally, the leaves of the lower electrometer diverged, while those of the upper were not at all moved.

The electric signs do not come from the air; for the rod is not alike electrified throughout.

It is very remarkable that the nearer the contact is made to the upper point of the compound conductor to take its electricity from it, the greater is the positive divergence of the lower electrometer; and if the discharge be made very near the upper point, the electrometer will indicate *plus* but very feebly in comparison with that below.

Its state differs as the contact is made in different parts of its length.

If these two electrometers, the conducting parts of which form together a length of six feet, be raised perpendicularly some feet above the earth, the *plus* electricity will be observed in both; but if they be separated horizontally, the upper one will retain its divergence, and the lower will augment it suddenly, even to double. From this it is evident that the conductor has not the same charge of electricity through all its length, and this is precisely what is called polarity; and as the

Electric polarity.

presence

preſence of the electrometer is not eſſential in this experiment, we may conclude from it that each conductor placed perpendicularly on the earth, has its polarity in the direction in which we have juſt explained it.

One extremity of an electrometer indicates divergence; the other does not.

This ſhows why an electrometer indicates a divergence *plus* when the point is touched with the hand in the open air, and is ſoon withdrawn, while there is no divergence when the lower part of the wire is touched. This phenomenon is ſeen in Weiſs's electrometer, even when the wire is not more than three quarters of a foot long.

Experiments on the electricity of the atmoſphere uncertain.

From all this it will be ſeen how deceitful the experiments on the electricity of the atmoſphere may be; for in the common method, the hand is directed to be moved from the bottom upwards, towards the point of the electrometer, to deprive it of its accidental electricity, and it is preciſely this which communicates it to it.

The intervention of a cloud, rain, &c. reverſes the phenomena;

With reſpect to the exceptions which the facts announced above may be liable to, it muſt be obſerved that when a ſtorm drives a cloud over the zenith of the obſerver, or when rain, hail or ſnow falls at the place of obſervation, the phenomena which occur are totally oppoſite; the electrometer gives *plus* when it is brought towards the earth, and *minus* when it is removed from it, &c. &c.

but a continued rain or a ſky wholly obſcured by clouds does not.

But this anomaly is only a tranſient effect, and the pretended negative ſtate of the atmoſphere does not exiſt, either when the rain is of long duration, or when the ſky is entirely covered with clouds; for in the latter caſe the electrometer is poſitive on being raiſed, as when the ſky is clear.

Theſe changes occaſioned by the atmoſphere of the cloud,

The perturbations ſpoken of are only momentary and change continually. It is evident that theſe changes cannot be attributed to the charge of electricity ſet at liberty in the air: it is more probable that theſe maſſes of clouds modify the electrometer by their atmoſpheres, like the tree in the preceding experiment. The perpendicular conductor may be compared to an iron bar, the polarity of which is reverſed accordingly as the ſame pole of a magnet is preſented to one or other of its extremities; theſe effects of the meteoric maſſes may alſo be imitated by conductors; the ſole motion of the hand above the point of an electrometer is ſufficient to produce the negative ſtate in queſtion, and inſulation renders theſe effects more ſenſible.

and may be imitated.

(To be concluded in our next.)

SCIENTIFIC

SCIENTIFIC NEWS.

PROFESSOR DANZEL of Hamburgh made, in the beginning of laſt year, a ſecond experiment of the mechaniſm he has invented for the direction of aeroſtatic machines. He is ſaid to have obtained a progreſs, in a right line, of twelve feet in a ſecond, which is nearly equal to three leagues in an hour, although he did not bring more than half his means into action. On the ſame day he made an eſſay with another machine for the ſame uſe, the reſult of which was not leſs ſucceſsful. He has ſince publiſhed the principles of his diſcovery in a pamphlet, entitled, "Baſis of the Mechaniſm for the Direction of Aeroſtatic Globes, by Profeſſor Danzel, of the Society of Emulation of Abbeville, at Hamburgh."

Mechaniſm for directing aeroſtatic machines.

The claſs of phyſical and mathematical ſciences of the National Inſtitute of France, in its ſitting of 2d Pluvioſe, in the year XII. elected the following correſponding members. Meſſrs. Leblond, engineer, returned from Cayenne; Bernard, engineer and aſtronomer, at Bagnols; Simons, phyſician at London; Crell, German chemiſt; Thunberg, the ſucceſſor of Linnæus in Sweden, who made the voyage to Japan; Bugge, aſtronomer to the king of Denmark; Goſſe, chemiſt at Geneva; Prouſt, chemiſt at Madrid; Cugnoli, aſtronomer at Modena; Reboul, to whom we are indebted for the meaſures made in the Pyrenees; Mendoza, a Spaniard, ſettled at London, known by his important works on navigation. They were all correſpondents of the Academy of Sciences in 1793.

Correſpondents elected by the claſs of phyſical and mathematical ſciences of the French national Inſtitute.

The ſame claſs has prolonged the time for receiving anſwers to the following prize queſtions, until the 1ſt Germinal, in the year XIII. "To determine by experiment the different ſources of carbon in vegetables." And, "To determine by anatomical and chemical obſervations and experiments, what are the phenomena of the torpidity which certain animals, ſuch as marmots, dormice, &c. experience during the winter, with reſpect to the circulation of the blood, reſpiration and irritability; to enquire what are the cauſes of this ſleep, and why it is peculiar to theſe animals."

Prize queſtions.

The value of the prizes is doubled, and conſiſts of two kilogrammes of gold, about 283*l.* ſterling each.

The aſtronomical prize, inſtituted by M. de Lalande, to be given to the perſon, in France or elſewhere, the members of

the

Prize queſtions.

the Inſtitute alone excepted, who ſhall have made the obſervation moſt intereſting, or publiſhed the memoir moſt uſeful to the progreſs of aſtronomy, was in the public ſitting of the 6th Meſſidor, decreed to M. Joſeph Piazzi, Profeſſor Royal of Aſtronomy and Director of the Obſervatory of Palermo, for the work which he has lately publiſhed under the title: *Præcipuarum ſtellarum inerrantium poſitiones mediæ ineunte ſæculo XIX. ex obſervationibus habitis in ſpecula Panormitana. Panormi*, 1803, one volume in folio.

The ſubject of the mathematical prize propoſed by the claſs, in this ſitting, is the following: To give the theory of the perturbations of the planet Pallas, diſcovered by M. Olbers." The works are to be written in French or in Latin; and are to be received to the 1ſt Germinal in the year XIV. The prize is a gold medal weighing a kilogramme.

The academy of ſciences, literature, and the fine arts of Turin has propoſed the following new prizes.

Claſs of Phyſical and Mathematical Sciences.

Firſt Prize. The electric and galvanic fluids offer ſo many points of analogy, and ſo great a number of different effects, that many philoſophers believe them to be identical, and many others make them two diſtinct fluids:

New experiments are required which ſhall decide, in a definitive manner, on their identity or diverſity.

Second Prize. It will be ſeen in the *Connoiſſance des tems*, for the year XII. page 217, that the refractions which are adopted do not ſhow an agreement in the obſervations of the ſummer and winter ſolſtices of the years 7, 8, and 9, to give the ſame obliquity of the ecliptic, as they ſhould do; and it is clear that a difference, ſuch as is found, of eight ſeconds in the reſult of the calculations, not of one obſervation, or of two, but in the total of ſeveral, made on different days of different years, muſt have ſome cauſe:

A ſatisfactory explanation is demanded.

Claſs of Literature and the Fine Arts.

Queſtion propoſed. *To demonſtrate whether the economical ſcience known by the name of* Statiſtics, *is a new ſcience, and what are the advantages ſtates may derive from it.*

The prize for each queſtion is 600 francs; and memoirs are to be received until the 30th Frimaire of the year XIII. They are to be written in Latin, French, or Italian, and

ſent, free of poſtage, to the academy. The prizes will be declared in the laſt public ſitting (Meſſidor) of the ſame year. Prize queſtions.

The ſociety of emulation of Colmar in its ſitting of the 17th Thermidor, propoſed the following ſubject for a prize of 300 francs, to be decreed in the public ſitting of Meſſidor, in the year XIII.

" 1. What are the readieſt and leaſt expenſive means of converting into animal oil, ſuch remains of animals as have been hitherto loſt to the arts and to conſumption?

" 2. To what arts, and to what kinds of fabrication can this product be applied; and what preparations ſhould this ſubſtance undergo before it is brought into commerce?

" 3. Finally, in an extenſive undertaking, the object of which would be to convert the remains of animals into animal oil, what precautions would be neceſſary in the adminiſtrative police, to prevent the public health from being injured?"

The memoirs are to be written in French, German, or Latin, and ſent before the 13th Meſſidor, free of poſtage, to the Prefect of the Department, or to the Secretary of the Society.

Extract of a Letter from Mr. J. DALTON, *Lecturer at the Royal Inſtitution. Dated, Mancheſter, March* 12, 1805.

A VERY remarkable and ſingular appearance of the *aurora borealis* was obſerved at Mancheſter from 10 to 11 in the evening of Saturday the 23rd of February. The ſky was almoſt completely obſcured with dark clouds, eſpecially to the ſouth, with ſome rain; about 60° above the horizon, on the ſouth meridian, there was a ſpace in which the cloud was leſs denſe, where a very ſtriking vacillating flame arreſted the attention of moſt people who happened to be out. It ſometimes ſhone with ſuch vividneſs as to exhibit a ſtream of light right down to the horizon through the thickeſt part of the cloud; and at other times, the whole ſouthern region was illuminated as with a flaſh of lightning. The light ſeemed ſcarcely to reach the zenith, and at breaks in the clouds to the north, no light appeared. Should this phenomenon have been obſerved in the ſouth of England or in France, a compariſon of the obſervations may lead us to form ſome idea of the height of this and other appearances of this meteor, which occur much more rarely of late years than formerly. Remarkable aurora borealis.

To

THE ingenious inſtrument of I. S. F. *for exploſions has not appeared to the Editor to be wanted. Chemical lecturers in this metropolis uſe a ſmall particle of phoſphorus, of the ſize of a pin's head, and ſprinkle upon it a pinch of the oxymuriate of potaſh, in a Wedgewood's mortar. This ſmall quantity, being ſtruck by a ſudden rub of the peſtle, gives a loud exploſion with the* viſible *flame, but the burning matter is too little to endanger the ſpectators.*

The excellent perſpective inſtrument deſcribed and drawn by the Rev. Wm. Gregor, *has no doubt the priority in actual date to that of* R. L. Edgworth, *Eſq. deſcribed in our firſt Vol. page* 281. *But he will ſee, by turning to that article, that the inſtruments are too nearly alike to admit of a ſecond publication.*

I am much obliged to Mr. Cumberland *for his favour of the* 17*th Feb. The uſeful objects mentioned in his poſtſcript appear to me to be ſuch as would be very acceptable to the public. I am afraid that the circumſtance of the invention of Mr.* J. *having been publiſhed before would make it leſs proper to be reprinted in a Journal which is honoured with ſo much original matter, that preſſes for inſertion. But I ſhall be happy to give it a more deliberate conſideration if his friend will favour me with the loan of the plates, and ſuch additional obſervations or notes as he may think fit to make.*

The caſe of deafneſs, communicated by Mr. Hill *of Wells, Norfolk, in which ſo much benefit was derived from electricity, is very intereſting. Some medical caſes have appeared in the early numbers of the firſt ſeries of this Journal; and others preſented themſelves for my deciſion as Editor with regard to their value and importance. But in the attempt to perform this part of my duty, I found difficulties ſo great, and a ſubject ſo extenſive, that it appeared proper to confine the papers, to be admitted, to ſuch as are directly and immediately connected with chemical and mechanical ſcience. I ſhall be happy to convey his letter to a reſpectable Medical Journal, if he ſhould think fit.*

I am very ſorry that the letter from a Correspondent, *who requeſted ſome enquiries to be made, concerning the fundamental experiments on heat, of the late* Dr. Irvine, *has been ſo long without its effect. I hope to be enabled to give the narrative he requeſts in the next number.*

Fig. 1.

Fig. 2.

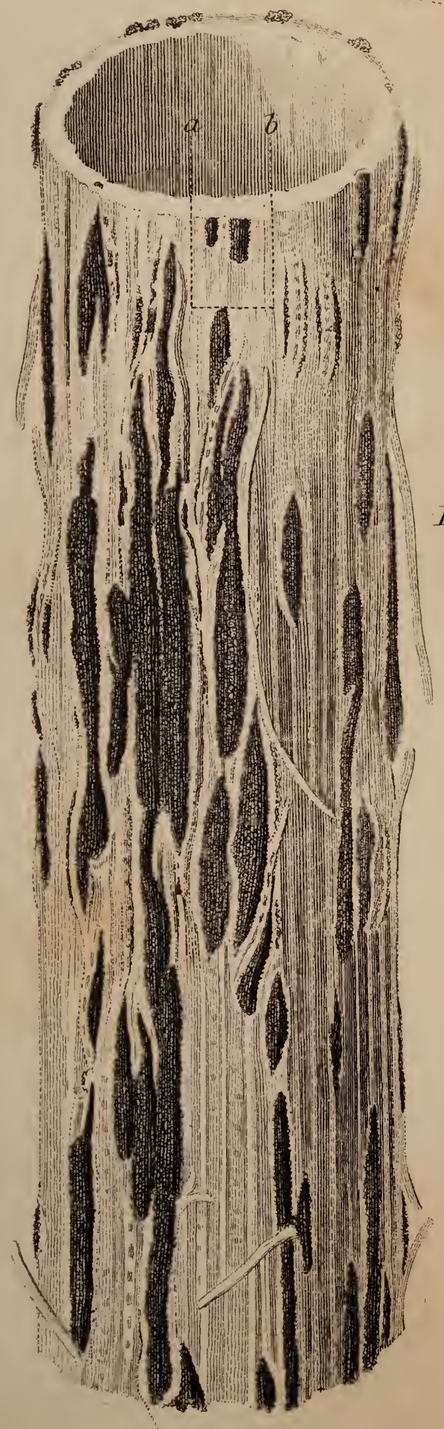


Fig. 3.

Magnified Representations of the Parasitic Plant which causes the Blight in Corn.

Fig. 4.

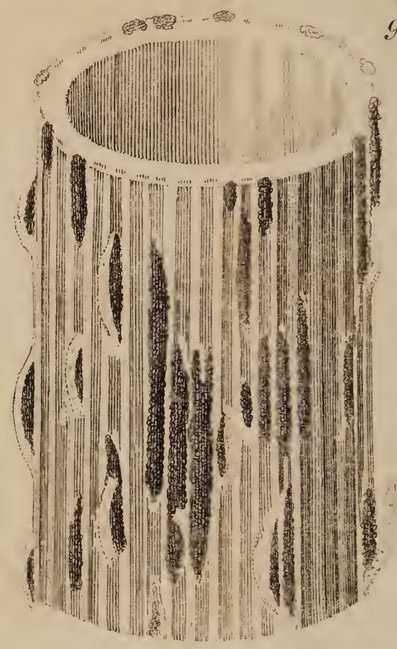


Fig. 5.

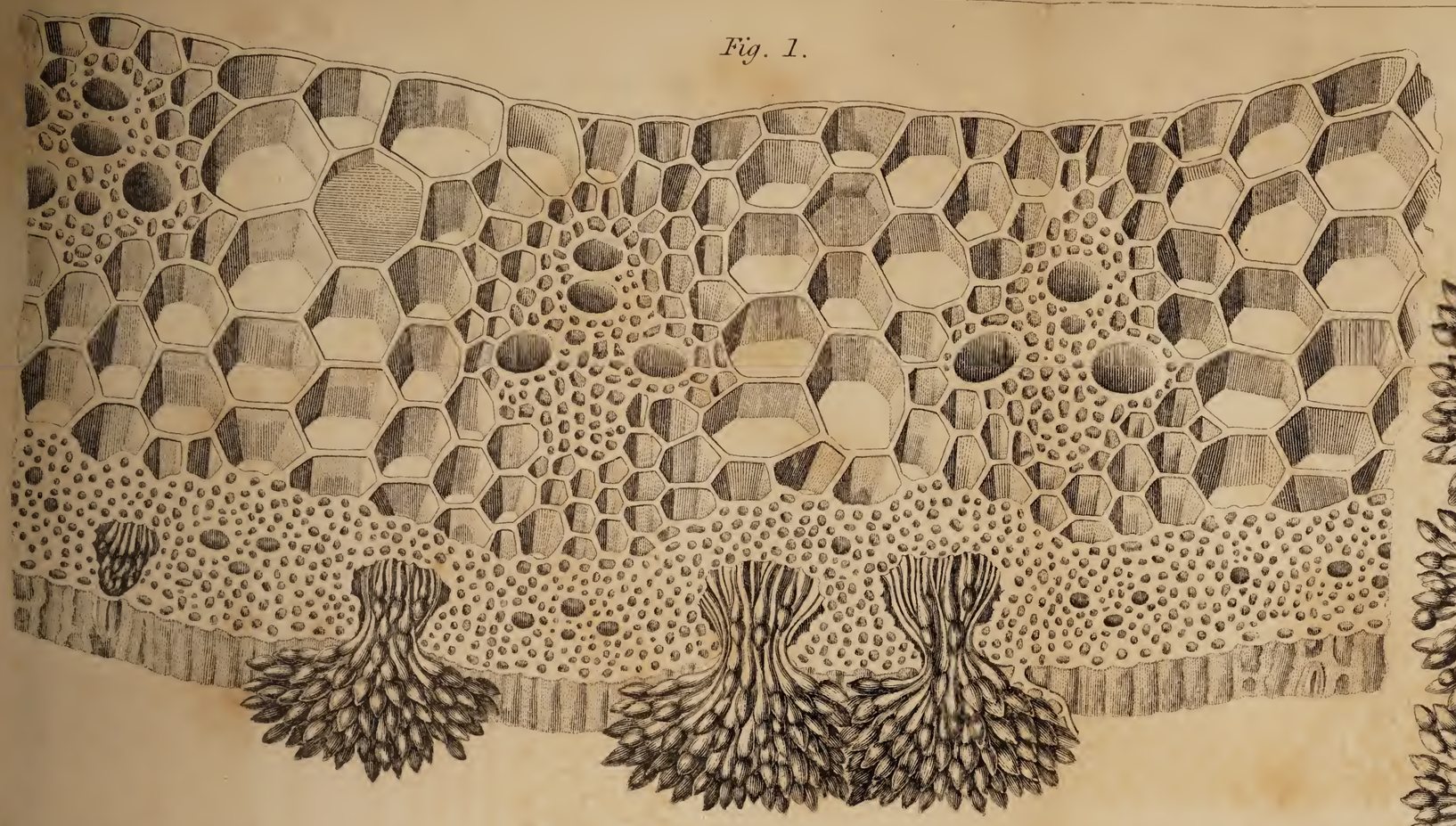
Fig. 6.

Drawn by Francis Bauer.

Engraved by Mulleaux Boydell &c.

Philos. Journal Vol. XLIX. p. 304.

Fig. 1.



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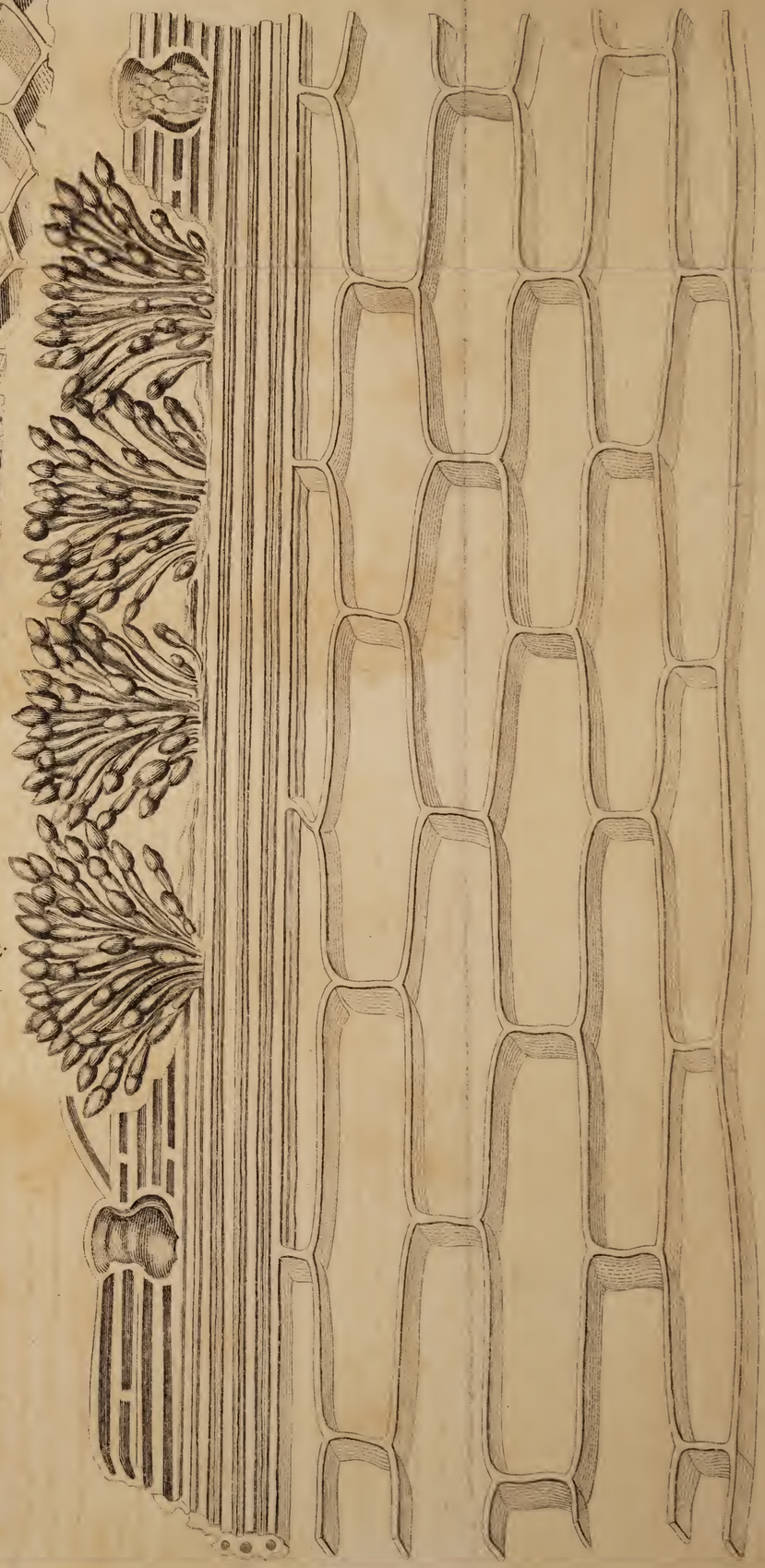

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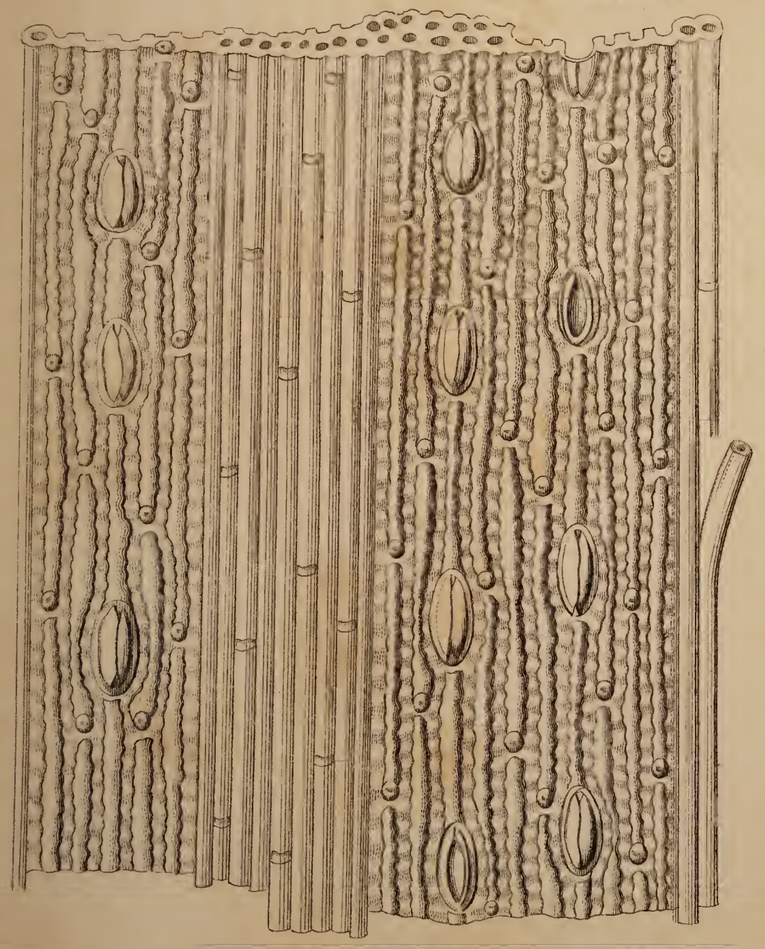


Highly magnified Sections and Structure of Wheat Straw; exhibiting the microscopic plant which occasions the Blight in Corn.

Drawn by Francis Bauer

Engraved by Mutlon Rufsell Co.

Fig. 1.

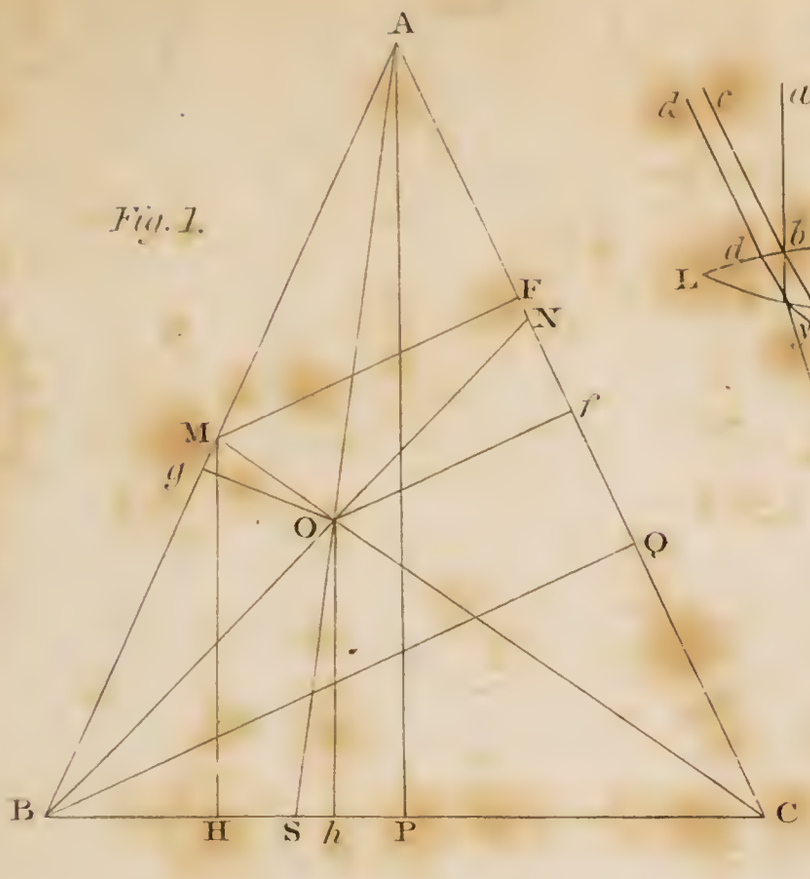
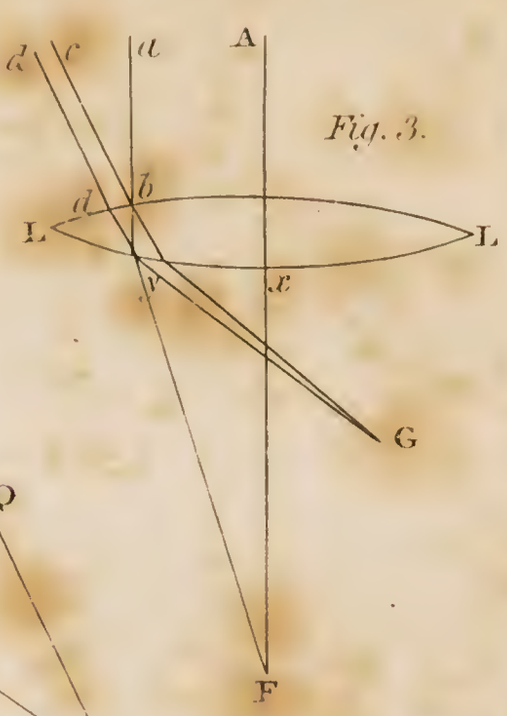


Fig. 3.



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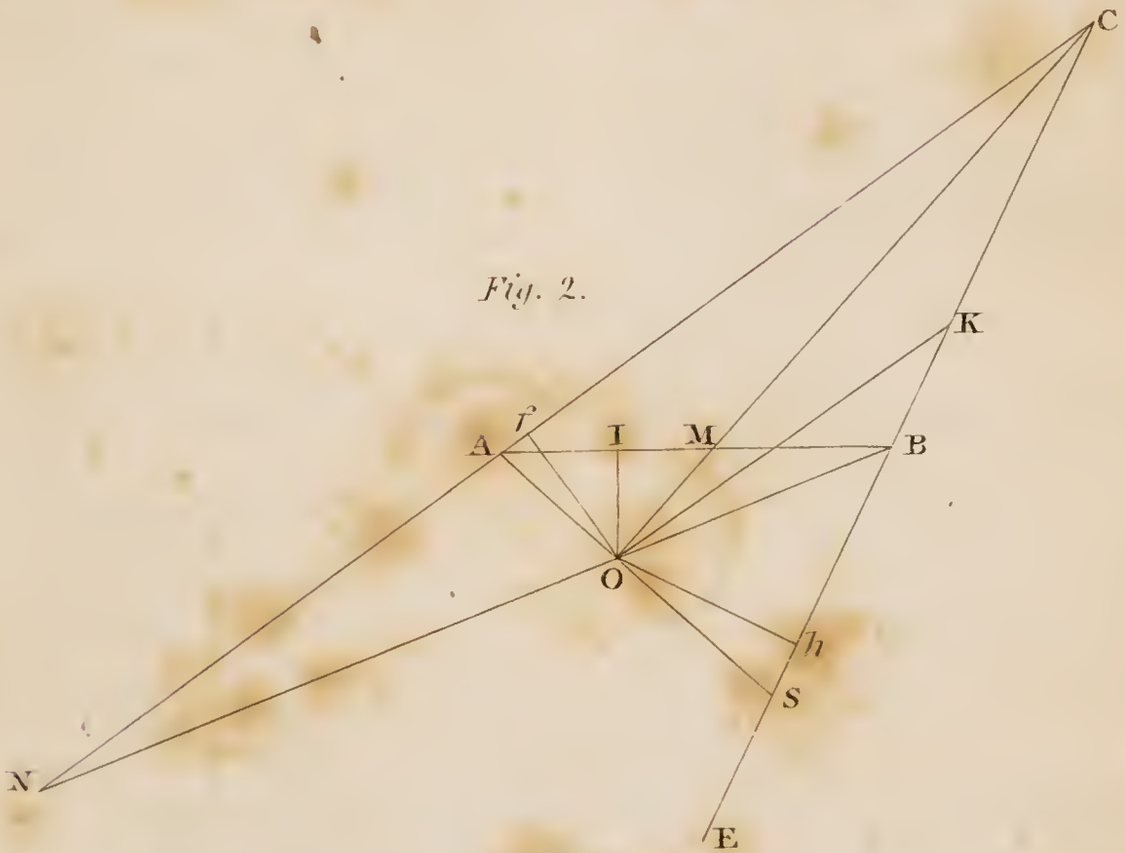
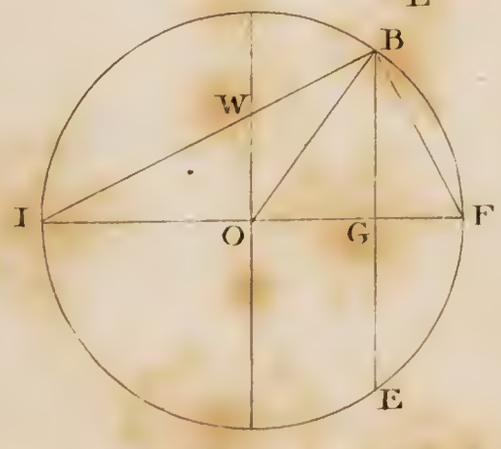


Fig. 4.



Mallow & Russell Col

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